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INDUSTRIAL
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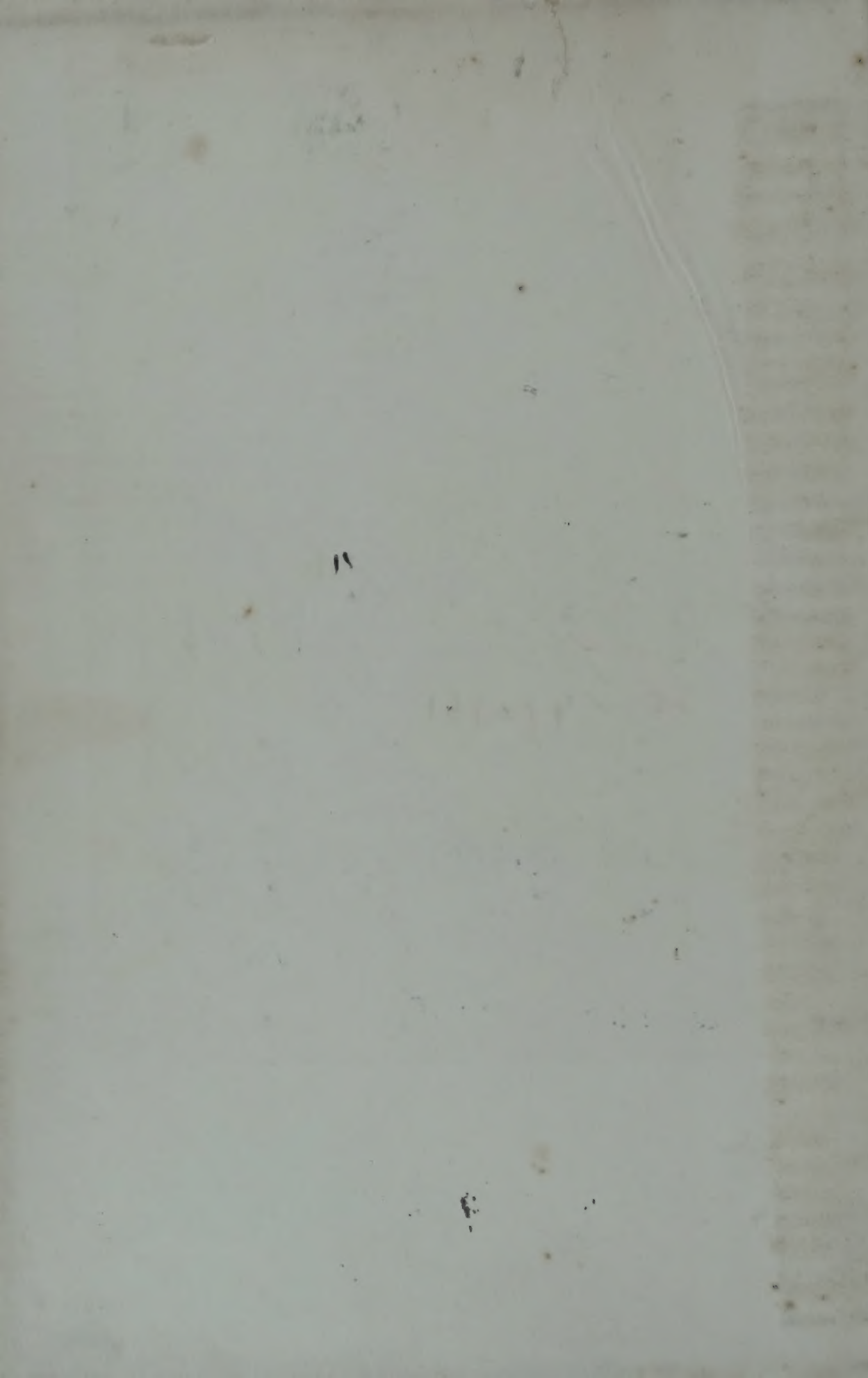
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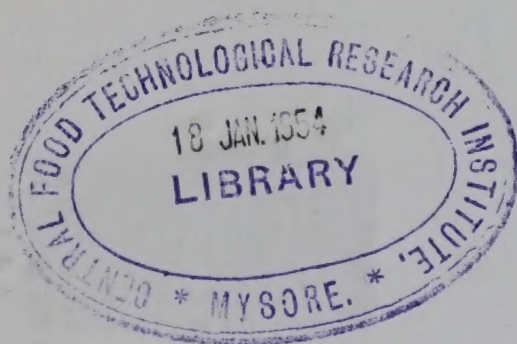
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THE THEORY AND PRACTICE OF INDUSTRIAL RESEARCH

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Theory and pract.

To

BARBARA VALENTINE HERTZ

Without whose patient assistance this book
could not have been written

The constitution of nature is as it is. . . . Things and actions are what they are, and the consequences of them will be what they will be; why, then, should we desire to be deceived?

—BISHOP BUTLER (Sermon VII)

PREFACE

The application of the methodologies of the scientific disciplines to industry has been comparatively recent, and its intensity has increased markedly only during the past few decades. Prior to 1900 the use of technologically creative resources in the industrial field was of a relatively minor nature. It is perhaps understandable that the attention of students of industrial management has not heretofore been directed toward this area to any great degree. On the other hand, at the present time, nearly half of all the scientifically trained personnel in this country are employed in industry.

It is, therefore, inevitable that the necessity for efficient use of this limited reservoir of creative ability should arise. A prime objective of workers in the field of organization and administration should be to undertake such analyses as may be required to extend the basic principles of sound management to such areas. Although much has been achieved by such eminent researchers as Mees, Boyd, Holland, and others, and by such organizations as the Industrial Research Institute, few studies have been undertaken with the object of isolating the elements of the process analytically, and then synthetically constructing an industrial research methodology.

The present book undertakes this task. It contains the outlines of a proposed methodology whose objective is to make plausible the view that the process is susceptible to empirical, logical planning, without undue restrictions upon the creative talents involved. It is hoped that it will thus partly fulfill one of the basic goals of industrial management—the reduction of inefficiency in industrial processes.

It assumes that research is susceptible to rational understanding. The industrial research process represents an amalgamation of scientific creative techniques with economic motivations. The research administrator and the industrial manager have mutual responsibilities in assuring the profitability of the procedures used to solve problems. Management must arrive at a sound

policy with regard to project selection, and research administration must objectively scrutinize proposals and the progress of its own work. The entire organizational pattern should be such as to reinforce the creative abilities of the personnel. The main contribution that this study makes in these regards lies in the direction of education toward more objective ways of thinking on the part of the industrial technologist as well as the managers of business. Research in industry cannot be considered apart from its total environment if it is to operate efficiently. Consideration of *all* the available data and potential methods is as essential here as it is in resolving other types of research problems. If this book has outlined the need for (and some methods of accomplishing) this mutual understanding, it will have served its purpose.

For those students of engineering and science who may enter the field of research, as well as for those who are engaged actively in this pursuit, this work is intended to present the essentials of the process. The material has been used successfully in the course, Administration and Organization of Industrial Research and Development, given in the Department of Industrial Engineering at Columbia University. It is hoped that educational and industrial institutions will recognize the general need for the teaching and understanding of this subject in our modern society.

The first four chapters present theoretical, background, and analytical material on creative mentalities, problem solving, and scientific method. The remainder of the book attempts to apply these concepts to industrial research, proceeding from such general topics as the industrial background of research and the genesis of a research program, to specific areas of interest in this field. While the theoretical material is not essential to the reading or study of the remaining chapters, it should be pointed out that an understanding of *why* the researcher does what he does, and *how* he does it, is invaluable in administering and directing his activity. The author will be grateful for any criticisms or suggestions regarding the material presented.

The study was originally conceived as a doctoral dissertation under the direction of Professor Walter Rautenstrauch, Department of Industrial Engineering, Columbia University, who has served as a continuous source of inspiration and guidance. Pro-

fessors R. T. Livingston and S. B. Littauer of the same department, R. P. Piperoux, Director of Engineering Research of the Celanese Corporation of America, and F. M. Culpepper have had especial influence in coordinating and modifying the ideas contained herein. The author is indebted to many other academic and industrial associates for their numerous contributions and suggestions. To his wife, to R. Dederich of Hofstra College, to Helen Valentine and Dorothy Dziuban, goes credit for the careful editing and preparation of the manuscript.

DAVID BENDEL HERTZ

NEW YORK, N.Y.

August, 1950

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CHAPTER I

INTRODUCTION

Definition of Research

The advance of technological development and the resources being utilized to increase the pace of scientific study in the past few decades are only two of the reasons for being concerned with problems of understanding and managing research. Is there some underlying pattern in research activity which can be useful to those engaged in this work? What portion of the labor expended in institutions and industrial research laboratories is being wasted because of a lack of understanding of the fundamental aspects of this type of work? Such questions indicate the bare recognition that there may be a difference between the methods used to manage and control man's other social and industrial institutions and those which should be used to administer this field of recognized individuality.¹

While the techniques for managing the productive capacities of machines and processes have become increasingly dependable and predictive, methods for managing research activity are perhaps little more effective than they were in Liebig's laboratory in 1825. And this is despite the development of organizational and management methods, theories of budgetary analysis, and other agencies for administrative control. Yet if man is to survive with a continual increase in his standard of living, in a world in which he is rapidly depleting his natural resources, sound direction of research is as important as an increased capacity for managing the men and machines of production. As the Industrial Research Institute points out,

¹ It is as though we formed an industrial organization composed of only top executives and their staffs. Ordinary administrative procedures would probably produce chaos. There were several examples of this difficulty in the top-level planning agencies in Washington during the Second World War.

It is of interest in this connection that, while the technical results of industrial research have been published in tremendous volume, very little has been printed about the *management* of research. Yet the growing volume of work done and money expended certainly warrant some study of research management techniques.

As a basic premise, research may be defined as the *application of human intelligence in a systematic manner to a problem whose solution is not immediately available*.

This is intended to be a broad definition and to cover the solitary worker, the informal group, and the organized team, working in any field of human endeavor where human intelligence is required for the solution of specific problems.

This book is written with the view that the *pattern* of organizing and administering a research group, in this broad sense, is capable of rational analysis and understanding and, when discerned, is applicable to increase the efficiency of research in general.

Group research should be distinguished from individual research, although the two are in some respects similar. The need for integration and the systematic approach is a minor consideration when applied to the lone worker, but as soon as two or more individuals combine to solve a problem, the need becomes paramount.

As Killeffer puts it, "Research can be considered a game governed by certain rules. A close examination of the rules and the application of the general principles derived from them are certain to improve one's chances."²

For the *individual* research worker, a group research methodology refers to his efforts to *influence or cooperate* with others, while a system of scientific logic is being used to guide his own work in accord with some previously determined *goal or purpose*. This goal is usually determined by the environment, laboratory, or enterprise in which he works. It should be apparent that the scientific work pattern originates in and is derived from *educational* influences, self-inculcated or disseminated in the usual collective study groups.

For example, the laboratory chemist may follow a careful plan

² D. H. Killeffer, *The Genius of Industrial Research* (New York: Reinhold Publishing Corporation, 1948), p. 6.

of hypothesis, experiment, theory, etc., which was taught him in his graduate work in chemistry or in his early laboratory apprenticeship. Or he may follow a less skillful plan, also the result of early training. On the other hand, in group work, the guidance of a skillful director may render the individual's methodology less important; conversely, poor direction in research can waste talent most effectively. It will be necessary to treat individual and group methodology separately, but their close relationship should be recognized.

Motivations and End Results

Research methodology should not be confused with either *motivations* or the *value* of the end results. To say that a given research worker analyzes X-ray diffraction patterns because he is "searching for the truth" is to make an unnecessary oversimplification. Most probable motives are apt to be best studied psychologically, and the value of the results of such studies in developing a comprehensive scheme of research planning and administration is dubious. It should be admitted, however, that the specific direction in which a scientist desires to work and in which he accomplishes the greatest amount of work, is influenced by his over-all environment. As Dr. Sarton points out in his *Introduction to the History of Science*, great men make great scientific discoveries, but the kind of research they conduct is determined by cultural influences of which they are themselves not aware. If specific knowledge of this type were available, it could be used in directing scientific endeavor along socially desirable lines. Studies have been made of various types of creative environments. These have considerable utility in outlining patterns for maximum research efficiency. We shall not be concerned further with motive but may reasonably state that the research worker seeks, and probably often finds, sufficient satisfaction in his work to enable him to continue to pursue it conscientiously.³

³J. R. Steelman, *Administration for Research*, Vol. 3, *Science and Public Policy* (Washington: U.S. Government Printing Office, 1947), Appendix III, "Opinions of Scientists about Their Work," pp. 205ff. Indicates that among approximately 600 scientists, intellectual satisfactions and temperamental gratifications are the primary (divulged) motivations in

In so far as a rationale of research method is concerned, the same type of confusion is engendered by the widespread implication that the social or industrial value of research is in some way connected with the *manner* in which it is conducted. Based on the premise that there are best methods of solving problems,⁴ it appears reasonable to conclude that the use to which the solution is put socially or industrially can bear no a priori relationship to the method. It is also clear that no valuation of the results of specific research can be validly projected beyond the immediate future. Even immediate evaluation of results is difficult, as we shall see.⁵ However, it is relatively simple to visualize intuitively the whole of research advancing on a broad front and increasing the general store of human knowledge. It is even possible to treat the increase of knowledge more quantitatively and make some rational predictions as to the over-all trend. Such a development of the topic does not lie within the scope of this book.

If an optimum set of conditions governing the research process can be shown to apply to all human activity falling within the limits of the definition stated previously, then there is no necessity to consider the merits of the various definitions of "pure," "basic," "fundamental," "applied," "industrial," etc., which are so widely discussed in the literature on the subject.⁶ In particular it would appear desirable to refute the implication that "technological" or "applied" research, assuming it to be

pursuing their careers. Social values of their work are a strong secondary source of satisfaction, while economic return and prestige are rated considerably lower than the other satisfactions. This survey is treated in greater detail in Chapter VIII.

⁴ Problem solving is a necessary, but not a sufficient, part of the definition of research, given above.

⁵ Cf. B. Lovell, *Science and Civilization* (New York: Thomas Nelson & Sons, 1939), p. 50. "In any given time and place of science, one is always coeval with the enunciation and verification of principles embracing masses of new knowledge, but the relation of these to the past, and their future significance, is not always obvious."

⁶ J. R. Steelman, *The Federal Research Program*, Vol. 2, *Science and Public Policy* (Washington: U.S. Government Printing Office, 1947), pp. 311ff. Numerous definitions of research by various authorities are listed, and division is made into categories of (1) fundamental research, (2) background research, (3) applied research, and (4) development. The general character of these definitions should be apparent.

carried on systematically, is in some way inferior to other forms of "scientific" research.

If a given human activity is research according to any definition, then the comprehensive methodology accorded to it logically applies, although techniques, apparatus, and environment may differ. Qualifying adjectives can perhaps serve the purpose only of cataloguing the end results or of assigning problems to those environments where the apparatus or techniques are most suitable. This is more a problem of choice of resources than a transformation of some variant methodology. Lovell makes this point when he says,

Such technique [in utilizing the scientific method] is not differentiated in "practical" or "theoretical" science, for all aspects of a science are interdependent. . . . The continually widening scope of science has led to a position where no one man can understand fully more than a single branch of any particular subject. We have not only the separation between the biologist, mathematician, and physicist, but further separation of men within each subject, and the final separation of men in one aspect of a subject, into "theoretical" and "practical" men. The relation between these two must essentially remain very close, otherwise the work of neither has meaning. There exists nevertheless this regrettable [and unnecessary] separation of the workers which has led to neither appreciating the difficulties of the other. . . .⁷

John Dewey in *The Quest for Certainty* has pointed out that "There is . . . no *a priori* test or rule for the determination of the operations which define ideas. They are themselves experimentally developed in the course of actual inquiries. They originated in what men naturally do and are tested and improved in the course of doing." The fields in which research operations are undertaken cannot be circumscribed by any preconceived value considerations but are bounded only by the limiting definition. It is probably true that "the researcher in pure science is free to explore any bypaths that may attract him; while the worker in applied science must continually direct his attention and confine his energies to achieving his specified objective."⁸ However free or limited application to specific problems may be,

⁷ Lovell, *op. cit.*, pp. 47-48.

⁸ Killeffer, *op. cit.*, p. 12.

recognition of an underlying pattern common to all research activity is not necessarily altered thereby.

Research and Science

The close relationship of research and science has been recognized and thoroughly explored in the past. As a result of this association, the clear distinction essential to a study of research methodology has been neglected. Science is not research, although research must be used to advance science, and the "scientific method," while a part of research, is not necessarily synonymous with it.

Science may be defined as ordered knowledge of natural phenomena and of the relations between them. "All peoples, even the most primitive, have a considerable accumulation of knowledge, which is essentially scientific, and make practical application of at least some parts of this knowledge."⁹ Science thus defined as a body of systematized information is passive; of itself it cannot add to the accumulation. Action must be undertaken to acquire further knowledge, and one form of such action is research, as herein defined. "Science emerges from the other progressive activities of man to the extent that new concepts arise from experiments and observations, and the new concepts, in turn, lead to further experiments and observations."¹⁰

Research has been defined above as the application of human intelligence in a systematic manner to a problem whose solution is not immediately known. Such action may lead to an increase in the body of organized knowledge, but this is not a *necessary* adjunct to the definition. The observation and recording of data may advance science by the accumulation of information, but unless utilized to solve certain types of problems (to be defined) they are not research. In general, the method of pure observation and recording alone has not been seriously followed by physical scientists. The importance of the problem concept in studying the pattern of research is treated in detail below. Science is not necessarily dependent on research, but practically

⁹ J. Roucek, ed., *Social Control* (New York: D. Van Nostrand Company, Inc., 1947), p. 168.

¹⁰ J. B. Conant, *On Understanding Science* (New Haven: Yale University Press, 1947), p. 22.

considered, the largest part of knowledge accretions has been obtained through activity falling within this definition. The exploration of unknown *problem areas* is what scientists generally recognize as research. To rephrase Weaver,

Research is clearly a way of solving problems—not all problems, but a large class of important and practical ones. The problems with which *scientific research* can deal are those in which the predominant factors are subject to the basic laws of logic, and are for the most part measurable. Research is a way of organizing reproducible knowledge about such problems; of focusing and disciplining imagination; of weighing evidence; of deciding what is relevant and what is not; of impartially testing hypotheses; of ruthlessly discarding data that prove to be inadequate or inaccurate; of finding, interpreting, and facing facts and of making the facts of nature the servants of man.¹¹

Thus, research is dependent on science for the solution of a large class of problems; this does not bar research from using unorganized or ordinary knowledge (sporadic vs. systematic) in the solution of other types of problems. The requirement is the *systematic application of intelligence*; this does not necessarily imply the use of *a body of systematic knowledge*. The point is that, while research is not inseparable from science, the underlying approach can be identical whether or not solution of a particular problem will add to the store of organized knowledge. Lovell in *Science and Civilization* points out that

To discover exact knowledge is the essence of science and the first duty of the scientist. Exact knowledge appears as the result of protracted application of the scientific method, and is manifestly not the only kind of knowledge with which civilization is acquainted, neither is the scientific method the only method of discovering or instilling knowledge. . . . Research is the effort to gain knowledge by application of the scientific method to the universe or to parts of it.¹²

Although we may recognize a close interdependence, we may thus clearly distinguish research from science. *Science is a body of knowledge; research is a process of problem solving*. The

¹¹ W. Weaver, "Science and Complexity," *American Scientist*, Vol. 36 (October, 1938), pp. 536–544. Italics added. "Research" has been substituted for "science" in the first instance, and "scientific research" for "science" in the second. This is evidence of the confusion of terminology mentioned above.

¹² Lovell, *op. cit.*, pp. 44, 59.

commonly used "methods of science" are more correctly designated as methods utilized in adding to this accumulation; in short, the scientific method. Methods of research, on the other hand, are those used in a systematic manner in applying human intelligence to problems.

This book is indeed concerned with the scientific method, inasmuch as it plays the leading role in a systematic manner of problem solving. The use of the scientific method involves the application of certain classes of logic and design of experiment, but there is much more to the coordination and direction of the research process than an understanding of logic and statistical inference. Group research activity is a social process, and its study involves examination of the creative mentality, environment, philosophy, as well as societal motivations for supporting this type of activity.

Individual and Collective Research

The solution of research problems in modern society may be undertaken in a number of ways. As we have seen, a fundamental distinction is to be made between individual and group activity. The solitary researcher may be an individual who is attempting the solution of problems without assistance or collaboration, perhaps in a university or other institution, or as an entrepreneur attempting to exploit the results of his researches or inventions. These types of research workers have become scarcer as science has become increasingly complex.¹³ Such individuals may even be directly connected with large research institutions. However, it seems probable that only a very small number of all workers, engaged in what may be defined as research activity, work completely alone. Clearly, the individual worker has no concern with the complexities of organization and direction involved in the collective research process. He most nearly corresponds with Killeffer's description as being free to

¹³ J. D. Bernal, *The Social Function of Science* (London: George Routledge & Sons, Ltd., 1939), pp. 266-267. "In the early period of individual scientific research the freedom of the scientist, in respect to his work, was limited only by lack of material means. . . . The development of modern science has made such individual work in most cases not only inefficient but practically impossible."

explore any avenues that interest him or appear profitable to him. His operations are not useful for the study of organization or administration; nevertheless, the creative aspect of his work is held to be identical with that of other researchers and can be discussed in that connection.

The collective approach to the solution of research problems may be traced to the earliest tribal associations, and its development has culminated in the multivarious institutions devoted to that purpose in modern society. The earliest historical examples stem from the Egyptian priestly cults and the Grecian schools of philosophy, such as those of Thales (*ca.* 600 B.C.) and Pythagoras (*ca.* 550 B.C.). These were essentially colleges founded for the purpose of individual reformation and the synthesis of an ideal society by problem study based largely on the personality of a master-teacher. The growth of such institutions¹⁴ declines with the onset of the Roman Empire and, upon its decay, any tradition of collective research which had developed was largely extinguished. Considerable amounts of collective study were undertaken in the monasteries of Europe during the Middle Ages. Little of this work can be distinguished as being research.

With the expansion of trade and commerce and the subsidence of the scholastic outlook in the fourteenth and fifteenth centuries, the individual research worker began to flourish. The growth of means of communication, while slow, stimulated this developing interest in science by diffusing the newly acquired knowledge and hypotheses. The first large-scale *organized* body devoted to research was formed in Florence in 1657: the *Accademia del Cimento* (Experimental Society).¹⁵ This was fol-

¹⁴ This should not be construed as indicating that these schools were experimental, or utilized the scientific method. However, they were devoted to problem solution, and the importance of the results of their work is well known. Their influence is more fully discussed in the following chapter.

¹⁵ There were others, earlier, such as the *Accademia Secretorum Naturae* of Naples in 1560, which was soon closed by the Pope on the suspicion that it was dealing in the black arts. In 1603, the transitory *Accademia dei Lincei* was established. The *Accademia del Cimento* was founded by disciples of Galileo and flourished for a number of years. Publication of its annals (*Saggi*, 1667) had a profound influence on the thinking of the time. See H. T. Pledge, *Science since 1500* (New York: Philosophical Library, Inc., 1947), pp. 51-55.

lowed by the Royal Society in 1662, and by the Académie des Sciences in 1666. These were primarily loose discussion associations of amateurs in science, and the research work accomplished was still for the most part individual. However, the French Académie was "a cooperative laboratory for scientific research rather than a free association of scientific workers. The results of this cooperative work were of some value, but as a whole the method proved a failure and the most important discoveries were made by individuals."¹⁶ In any event, a definite movement was afoot for the establishment of such societies; they spread and flourished and were, without doubt, the forerunners of modern collective research.

From these societies, research spread to the universities and governmental institutions, particularly the astronomical observatories. The period of world exploration was under way and the demand for adequate astronomical information was keenly felt. *The embodiment of contemporary environmental aspirations in research activity is fundamental.* The French Observatory was established in 1671, the English in 1676. They were essentially research laboratories devoted to the problem of providing satisfactory data for navigational purposes.¹⁷

Industry was able to utilize the advances being made by the individual research workers, although there was "little systematic connection with the corporate body of science."¹⁸ However, there were many brilliant mentalities connected with trade and industry as well as with science, and there is no question but that individual research was flourishing in this area also. Under these circumstances, it was not until 1825 that Liebig established the first laboratory of chemistry.

¹⁶ C. E. K. Mees, *The Path of Science* (New York: John Wiley & Sons, Inc., 1946), p. 86.

¹⁷ It is interesting to note that the French Observatory has been described as "lacking direction" during its first 100 years. Boscovich, the Italian physicist, was appointed director of optics in 1773 by the King of France, a position he gave up because of "intolerable" conditions. The problems of research administration are not new!

¹⁸ Pledge, *op. cit.*, p. 102. "Yet the interconnection and the small scale of everything in those days must be remembered. When Dr. Beddoes, of Clifton, wanted to exploit oxygen as a curative stimulant, the chemical assistant he found was the young Davy, and the instrument maker, James Watt."

The trend away from individual to collective research has continued; the research laboratory has spread to industry, and among the types of collective institutions now devoted to the solution of problems there are

1. University laboratories
2. Industrial laboratories
3. Trade association and institute laboratories
4. Professional societies and institutions
5. Teaching foundations
6. Endowed industrial research institutes
7. Scientific and technical bureaus of government

Of these, the most important, from the standpoint of influence, are governmental, industrial, and academic laboratories.¹⁹

The development of the institutions of research has been briefly outlined to indicate their common origin. This study will indicate that all are faced with the same problems in accomplishing research efficiently. Even at this stage, it is apparent that it would be difficult to show differences in problems of coordination and direction among them.

Research and Society

These are the social institutions which this study will analyze, along with the individual personalities of which they are composed. The social development of these institutions will not be considered in great detail, except in so far as it bears on the problems of organization, administration, and coordination. However, it should be stressed that

... every piece of research carried out in any place or institution, whether it be in the universities, in industry, or in the government laboratories, is not distinct and separate, but merely a part of one machine—a machine which depends for its efficient functioning on a rational organization of the whole.²⁰

Lovell goes on to point out that this machine was working in a most “shoddy” manner at the time (1939). The impact of

¹⁹ Lovell, *op. cit.*, pp. 40–43. In the USSR, the *direct* intellectual supervision of all research is undertaken by the Academy of Sciences, established in 1724, which directs the work of over 1,000 institutions.

²⁰ *Ibid.*, p. 59.

society upon research, and of research upon society, is of great importance for the future of civilization. As noted earlier, it is most essential that research lead the way to replacement or substitution for the depleted natural resources of our times.

In the course of this study, an attempt will be made to demonstrate that, whatever the frame of reference (the environmental influences of the surrounding culture), research institutions can be organized and directed in a manner which is efficient from the standpoint of problem solving. Obviously this does not take into account whether research is exploiting the resources of nature or attempting to rehabilitate them. In a sense, exploitation has been the keynote of the past and has had the approval of the pragmatists, from Bacon onward. This has been of material benefit to society and at the same time has created problems of its own. Our future will depend largely upon the ability of the researchers to solve these problems and to reduce the *human* energy requirements for survival. There is a trend toward research in conservation, social problems, and allied constructivist fields.

The industrial economies of today recognize the importance of research for survival. A newspaper article written since the war pointed out that

Great Britain's economic future depends on an increase in her exports, this increase in turn on her industrial development, and her industrial development on scientific and engineering research. Here science and politics interlock. Parliament, therefore, created a Parliamentary and Scientific Committee which gives it advice and which consists of some two hundred peers and M.P.'s and the representatives of about eighty scientific bodies.²¹

As Hogben puts it,

Whether we choose to call it pure or applied, the story of science is not something apart from the common life of mankind. What we call pure science only thrives when the contemporary social structure is capable of making full use of its teaching, furnishing it with new problems for solution and equipping it with new instruments for solving them.²²

²¹ *New York Times*, Oct. 17, 1948.

²² L. Hogben, *Science for the Citizen* (London: George Allen & Unwin, Ltd., 1938).

"The science called into being by the needs of a society ultimately attains a status where it begins to criticize and analyze the organization of the society and its government." ²³

The so-called frustration of science, as described by Bernal, has its roots in this criticism. It is felt by some that all the human problems in contemporary society, such as poverty or disease, can be solved, or at least greatly mitigated, by the proper application of research. "The great value of applied science has led to a school of thought that argues that scientific discovery is only justified by its application and that scientific research should, in fact, be engaged in only when it can be applied." ²⁴ This leads directly to the question, what is the future of research?

This study will not attempt to show that all research can be socially planned; unpredictability of the significance of research results has already been touched upon. However, the general trend may be noted as being one of growth. There is no doubt but that even out of competitive and military research may come results of the utmost constructive value to mankind.

Research itself, not dependent on the motivations of the supporting structure, can make progress; statistically, the problems of greatest human importance would be more likely to yield to the efforts devoted to their solution, the greater the resources used to implement those efforts. *Research must be considered as a resource, which is probably not wasted if it is used to solve problems in an efficient manner.* It is, however, limited in quantity, and one of the goals of the future is the increase in research personnel. The only significant suppliers of research workers are the universities; the consumers are governments, industries, and the universities themselves—in effect, society. According to Lovell,

It is not surprising then to find the industries and the universities quite interdependent. Industry is completely dependent on the universities for its supply of trained research workers, and needs the fundamental researches of the universities which it may ultimately exploit. The fundamental discoveries of the universities are more and more dependent on heavier and more complex machinery which only industry can

²³ Lovell, *op. cit.*, p. 72.

²⁴ Mees, *op. cit.*, p. 62.

develop and supply. Industry is also the salesman, producing the final effort in a salable form.²⁵

This goes quite far; thus, even surgical techniques may be dependent on industry's ability to supply complex instruments, and certainly some of the most potent disease-combating drugs, such as penicillin, are dependent on heavy industry for their large-scale manufacture. The future of research cannot be considered apart from that of the society in which it is nurtured. The trend of knowledge and discovery, projected into the future, may solve problems ever more concretely connected with the survival of civilization.

The Pattern of This Book

In this study the elements of the research process are outlined as intelligences applied in some systematic manner to certain environmental situations with the objective of overcoming obstacles or problems. When the environment is that of an industrial organization, then the process is industrial research. The mental operations involved in solving these problems are creative, in the sense that "insightful" behavior is used in arriving at hypotheses connecting the relationships inherent in a given problem situation. The use of an orderly method in observing, manipulating data, and postulating such relationships is shown to be a necessary condition for research *as we have defined it*. Problems have not always been solved by such a process, and a brief historical sketch of creative behavior and the development of a philosophy of science will lead to the defining of research problems and the components of a systematic method. The limiting conditions imposed upon problems of research are such as to exclude both the *routine obstacles* encountered in the environment and *metaphysical questions* of ultimate truth. Technical considerations of the existence of both method and data to arrive at solutions will eliminate the former, while a requirement of reproducibility is intended to obviate considerations of the latter.

We shall find that the required method comprises (1) the abstraction of information from the environment based upon cer-

²⁵ Lovell, *op. cit.*, p. 63.

tain definite presuppositions, the necessary one being the assumption of causality, (2) the definition of a problem by means of an intuitive hypothesis, and (3) the manipulation of the information in a manner consistent with the presuppositions so that the conclusions reached would have implications of reproducibility. The conclusion, on which action may be taken, is considered a *sufficient* solution to the problem. Questions as to the meaningfulness of the problem or its solution will be avoided since the concept of action taken implies a measure of meaning. Considered in these terms, the over-all efficiency of the research process is defined as being dependent upon the qualities of a particular solution to enable an individual or organization to take satisfactory action, based upon the future results predicted by the solution. The available methods of manipulation of information to arrive at inferences or conclusions present a problem of choice in each case, and it is implied that further and more efficient means will probably be developed in the future. It is also implied that solutions reached by the various methods in a given instance will vary in efficiency. Whether these implications are in accord with philosophic criteria of "truth value" will not concern us.

On the basis of these elements, the conclusion is advanced that the collective attitude of a "research team," selected in such a manner as probably to fulfill the requirements of the problem, will generally produce the most efficient group of presuppositions from which to proceed to its solution. The selection of an entire set of resources for solving a given problem, including the personnel, methods, and equipment, in terms of an analysis and a continuing reevaluation of all the factors will be advocated. The importance of the cooperative or unified approach is emphasized.

The implications of these conclusions with relation to industrial research will then be explored. The important additional factor of economic motivation will be found to influence and modify the process elements in a number of ways, distinguishing industrial research from institutional work of a similar nature. The most notable modification occurs in procedure for *selecting* problems. No such question would arise within the general theory of research since it does not comprehend the necessity for any selection beyond the requirements of the bounding condi-

tions. It is found essential for efficient industrial research activity that the problems be selected on the basis of strategic factors concerning general competitive policy as well as tactical elements involving feasibility of solution, resources required, and the time necessary. A number of criteria are outlined for the selection of projects and programs from these standpoints.

The choice, development, and direction of personnel are then discussed as being the core of industrial research activity. Methods are proposed for improved practice in this regard. It is found that many of the theoretical and practical principles are convergent from the point of view of optimal efficiency in this area. Thus, the selection of the best fitted project team is in harmony with the desirability of obtaining efficient solutions and maintaining satisfactory control. The proper selection of projects in terms of economic and scientific conditions is found to be efficient and to satisfy best the desire on the part of the researcher for tangible results.

The organizational and financial requirements to establish and operate a research organization in industry are carefully considered. Proposals will be made for measurements of progress in attaining objectives. The analyses of progress measurement, budget and forecasting requirements, and other factors should be made through the use of best available information. It is felt that the application of the objective principles we shall outline will enable further refinements of measurement to be established with greater validity. The fact that the direction of research activity is generally considered so esoteric as to be beyond the usual rational organizational principles is recognized. Nonetheless, it is felt that administrative and organizational procedures erected upon the framework of an analysis of the elements of the research process will assist in increasing its efficiency.

Objectives

It is hoped that this book may serve as the basis for further objective studies by other workers in the field. Much remains to be done. Questions of problem classification with relation to specific systematic methods best fitted to yield efficient results are likely to prove a fruitful field of study. More precise definitions of relationships of research to the economics of business

enterprise and the over-all economy are most desirable. Personnel studies, accounting methods, project evaluations—all these and more are deserving of intense attention if this relatively new segment of industry is to continue to grow healthily and efficiently.

We may agree with Kelley that "Research that is worthy the name is the most difficult task that society has differentiated out from the total field of human activity and called upon certain of its members to perform."²⁶ It is also hoped that the analysis of the research process and the synthesis of a rational methodology for application to organized endeavor in this study will reduce these difficulties by some finite amount and point the way to an increased probability of success in the field.

²⁶ T. L. Kelley, *Scientific Method* (Columbus, Ohio: The Ohio State University Press, 1929), p. 3.

CHAPTER II

THE CREATIVE MENTALITY AND RESEARCH PROBLEMS

Intelligence

Following the premise that research may be defined as human intelligence systematically used in problem solving, we shall attempt to synthesize a methodology for the organizing and administering of collective research activity. The implications of the definition leading to this synthesis indicate that the utilization of method based on a rational synthesis can increase the efficiency of research organizations.¹ In general, we are interested in the collective rather than the distributive aspect of this efficiency, as applied to a given research organization, not to a particular research worker. However, the proper application of *collective* intelligence depends to a large extent upon an understanding of what is meant by "intelligence." If the definition of research is to be clearly understood, it is necessary that the concept of intelligence be considered. This will lead to an investigation of the anatomy of a problem, and more specifically to a definition of research problems.

Intelligence is generally considered to be synonymous with understanding, and in some instances with all epistemic processes. The concept, as commonly employed, implies the satisfactory utilization of personal and environmental factors in purposive behavior. A distinction, important from the research standpoint, can be made between "perceptual" and "conceptual" understanding or apprehension. Perceptual apprehension involves the mental functions of sense perception. These involve the distinguishing of colors, smells, form, etc. According to Wolf, "the same processes . . . render possible our apprehen-

¹ The concept of efficiency in research need not be considered here; it is discussed at length below.

sion of similarity and difference . . . of local contiguity or difference, and of . . . simultaneity and succession.”² Most authorities agree that these perceptual functions should be excluded from a rigorous definition of intelligence, although the exact dividing line is subject to much debate.

For our purposes, a sufficiently precise description is that intelligence constitutes the ability to apprehend connections, where “connections” are used “in the sense of causal and rational (or logical) relations, or relations of causal and rational (or logical) interdependence.”³ Connections are not perceived in the same manner as the simple sense objects. They must be conceived or grasped, and translated (by what is often called “insight”) from a perceptual continuum of these ordinary relations into some conceptual pattern of causality. This is probably true of imagined objects and relations as well. It is apparent that sense-object data and imagined objects and relations form the raw materials upon which the research worker’s mentality must operate. Therefore, application of his intelligence to them implies the conceiving or apprehending of connections within the particular continuum by means of some ability we shall term “insight.” If we accept this description of intelligence and we conceive no other possible results of intellectual activity, then it may be said that intelligence is the ability to solve problems.⁴ We may describe this specific ability as “insightful,” in accordance with well-established terminology in the field. Therefore, some aspects of animal and human insight will be more closely examined. This will then introduce us to questions of the non-transformability of insight under differing environmental reference frames, the relationship of insight to the problems *available* within such frames of reference, and the classification of problem types.

Problem Solving

The ability to solve problems is not restricted to man alone. Experimental psychological studies of the behavior involved began with observations of animals, out of which have grown

² A. Wolf, “Intelligence,” *Encyclopaedia Britannica*, Vol. 12, 14th ed.

³ *Ibid.*

⁴ This presupposes a satisfactory definition of a problem in terms of the apprehension of causal relations.

examinations of human mental processes. The capacity of certain animals to resolve actively various types of problem situations, when given a desirable goal, has long been recognized. Early experimental work centered on the question as to whether the resolution was by trial and error, or by insight. If the former is taken to be the case, then behavior "is not controlled by any explicit perception of the relationships involved, [and] mastery is attained only gradually."⁵ The factors involved in the trial-and-error solution are association, conditioned response, repetition, and chance success. That some problems may be partly or wholly solved by animals or humans, and that the learning process may proceed in this manner, may readily be granted without detracting from the importance of final insightful solutions. Trial-and-error solutions can be connected with certain types of "inefficient" research work. However, at this point, we wish to consider types of behavior which apparently exhibit insight.

Kohler, in one of his well-known experiments on the behavior of primates, set up the following problem:

He used several young chimpanzees. At the first trial with a single box, he assembled six of the animals in a room which had smooth, unscalable walls, suspended a banana from the ceiling, and put the box in the middle of the room two or three yards away from the lure. All six chimpanzees leaped repeatedly for the banana, but Sultan, *who in other tests showed himself the most apt*, soon ceased jumping, paced up and down, *suddenly* stood still in front of the box, moved it quickly toward the objective, climbed, jumped and secured the banana, taking only twenty seconds in this final continuous act with the box. He repeated the performance the next day.⁶

Basing his conclusions on similar experiments, Yerkes considered the following to be characteristic of the insightful problem solution:

1. Survey, inspection or persistent examination of problematic situation.
2. Hesitation, pause, attitude of concentrated attention.
3. Trial of more or less adequate mode of response.
4. In case initial mode of response proves inadequate, trial of some

⁵ R. S. Woodworth, *Experimental Psychology* (New York: Henry Holt and Company, Inc., 1938), pp. 746ff.

⁶ *Ibid.*, p. 758. Italics added.

other modes of response, the transition from one method to the other being sharp and often sudden.

5. Persistent or frequently recurrent attention to the objective or goal and motivation thereby.

6. Appearance of critical point at which the organism suddenly, directly and definitely performs the required adaptive act.

7. Ready repetition of adaptive response after once performed.

8. Notable ability to discover and attend to the essential aspect or relation in the problematic situation and to neglect, relatively, variations in non-essentials.⁷

We shall find that this description is readily adaptable to human behavior in problem solving, and it will be of assistance in setting up guideposts for the research process. One further experiment, illustrative of insightful behavior, is worthy of note as described by both Polya and Woodworth:

A fence forms three sides of a rectangle but leaves open the fourth side. We place a dog on one side of the fence . . . and some food on the other side. . . . He may first strike a posture as if to spring directly at the food but then he quickly turns about, dashes off around the end of the fence and, running without hesitation, reaches the food in a smooth curve. Sometimes, however, especially when [dog and food are quite close to the separating fence] . . . the solution is not so smooth; the dog may lose some time in barking, scratching, or jumping against the fence before he "conceives the bright idea" (as we would say) of going around.

It is interesting to compare the behavior of various animals put in place of the dog. The problem is very easy for a chimpanzee or a four year old child. . . . The problem, however, turns out to be surprisingly difficult for a hen who . . . may succeed accidentally.⁸

The general viewpoint in this case is that the dog has shown insight, the hen none. The problem is apparently beyond the latter's capacity, except by trial and error. The obvious analogies to the case of human behavior will be discussed below. It should not be construed that a total absence of trial and error is implied in insightful solutions, or that trial and error is not useful in providing the necessary learning sets required eventu-

⁷ R. M. Yerkes, *Genetic Psychology Monograph* (1927), quoted in Woodworth, *ibid.*, p. 757.

⁸ G. Polya, *How to Solve It* (Princeton, N.J.: Princeton University Press, 1945), p. 203.

ally to solve problems by insight. The very appearance of seeming to apprehend the necessity for detouring around an obstacle, as evidenced by the dog's actions in the above example, is illustrative of some mental means for solving problems other than *physical* trial and error. For the purpose of studying the research process, it is not necessary to determine whether this insight is a rapid mental trial-and-error affair or a sudden enlightenment arising from the problematic situation. However, the evidence for the presence of the capability to apprehend connections is sufficiently great to enable its use in describing intelligence.

The premise here is that intelligence may be measured by insightful behavior, by the actual solving of problems, and can reduce the amount of physical trial and error required in a given situation. This has been supported by a great part of the psychological studies of human intelligence. Problems similar to those described previously have been used and are strongly indicative of the type of solution schema outlined by Yerkes. A typical experiment is the following, used by Durkin:

Her material consisted of flat construction puzzles, which proved to have several advantages in the study of problem solving. Everything was in sight; there were no hidden properties requiring to be learned by manipulation. The pieces were easy to identify so that "thinking aloud" and "retracing the solution" were comparatively easy. . . . [Five different puzzles were used, each consisting of four pieces which when assembled formed a square. The five finished squares were the same size.] The five small squares were given separately and after each had been solved once, the pieces from all were presented in a mixed assemblage with instructions to construct a Maltese cross from all the pieces. . . . The protocol of one [subject] who solved the large cross after experience with the small squares affords a vivid instance of the "flash" experience. This moment of insight or sudden reorganization was clearly dependent on the use of knowledge acquired in solving the small squares. More than that, the "flash" amounted to a realization that this knowledge could now be put to use.⁹

It will be seen that the "flash of genius," so widely discussed in the popular literature relating to inventors and creative thinkers, is no mere popular fancy. That some such process exists is the conclusion drawn by many authorities from such experi-

⁹ Woodworth, *op. cit.*, pp. 779-781.

ments and observations. The famous problem of the sum of an arithmetic series which Gauss is said to have solved by inspection at the age of six is another such example. A hypothetical reconstruction of Gauss' explanation of his solution is given by Wertheimer:

Young Gauss answered—of course we do not know exactly what he did answer, but on the basis of experience in experiments I think it may have been about like this: "Had I done it by adding 1 and 2, then 3 to the sum, then 4 to the new result [the problem was to find the sum of $1 + 2 + 3 + 4 + 5 + 6 + 7 + 8 + 9 + 10$], and so on, it would have taken very long; and trying to do it quickly, I would very likely have made mistakes. But you see, 1 and 10 make eleven, 2 and 9 are again—must be—11! And so on! There are five such pairs; 5 times 11 makes 55." The boy had discovered the gist of an important theorem.¹⁰

Such examples are indicative of the wide variations in intelligence of both degree or intensity of insight and range of facts which are colligated. The ability to learn from experience is certainly a part of insightful behavior at various levels. The entire process shows a gradation from complete lack of insight to an ability to abstract relationships from the physical terms connected. The latter ability is, no doubt, based on the totality of previous experience. The thinking process and reflective thinking are phrases used to distinguish this from the more simple type of problem solving. Such thinking may be defined as "active, persistent, and careful consideration of any belief or supposed form of knowledge in the light of the grounds that support it and the further conclusions to which it tends."¹¹ Whitney corroborates this thesis, pointing out that

Scientific thinking is in terms of carefully organized reflection. Likewise, the methods of the best research are found to be scientific in terms of accepted mind processes, involving all essential steps in problem solving. . . .

In fact, the careful thinker, whether in shop, office, or study, proceeds in terms of delayed action after a period of reflection, when evidence

¹⁰ M. Wertheimer, *Productive Thinking* (New York: Harper & Brothers, 1945), p. 90.

¹¹ John Dewey, *How We Think* (Boston: D. C. Heath and Company, 1933), p. 6.

on solutions can be carefully weighed. *This is the method of creditable research, in whatever field it is carried on.*¹²

The hypothesis of sudden enlightenment based upon the totality of past experience is in general agreement with recent theories of neurophysiology, which describe the storage of observations in terms of circulating impulses in the *neuron-synapses*¹³ circuits comprising the brain, plus the more or less permanent modifications in these elements which affect future operations. The brain may be considered analogous to some type of relay system where the neurons are "triggered" by certain combinations of messages (observations) fed to them by the conjunctive synapses. For this reason, analyses of the type suitable for feedback mechanisms may be used to describe its performance. Thus, with the proper combination of stored information and external stimuli, and perhaps internal and external resonance, the proper neurons may be "fired," and the problematic situation resolved extremely rapidly. This picture is deliberately oversimplified but is sufficient to indicate the general correlation of physiological with psychological theories of intelligence.¹⁴ In this case also, the variation in number, efficiency, and necessary modifications of the brain elements support the observed variations in degree and range of intelligence.

Although the general mechanism may be similar in all cases, it will be apparent that the insight suitable for one problem may not be adequate for even a modification of that same problem. This is probably due to the differences in "learning," or storage of information and modification of the brain elements, as well as to the variations in degree and range of intelligence. Hence, the chimpanzee who can solve the problem of a single button to open a door but who fails when two buttons are used; the child who can utilize an electric switch to obtain light but who could not possibly wire a room; the electrician who can wire an ordinary lighting circuit but cannot design a radio; the engineer who comprehends electrical engineering but not electrical

¹² F. L. Whitney, *The Elements of Research*, rev. ed. (New York: Prentice-Hall, Inc., 1942), p. 27. *Italics added.*

¹³ Neuron is an element of the nerve fibers. Synapses are areas of contact of neurons.

¹⁴ Cf. N. Wiener, *Cybernetics* (New York: John Wiley & Sons, Inc., 1948), Chap. V.

field theory; and the physicist who understands magnetic and gravitational field theories but cannot devise a general theory to subsume all observed phenomena.¹⁵ In other words, at each level of insightful ability there is a limit to the type of problems that can be solved, just as for a given electronic computing machine the complexity of problems which can be handled depends upon its design. There is no basis for the presupposition that any given insight is, or has been, the ultimate. On the other hand, there is also no basis for the more common presupposition that, given the proper conditions, human intelligence can solve any problem it can conceive.

A number of scientists and research workers have pointed out quite clearly that the extent of our ability to obtain experimental data concerning certain types of phenomena is definitely limited by the nature of the energy relationships involved. Such limitations lead directly to the possibility of our being able to ask valid questions, to which we cannot obtain equally valid (verified by experiment) answers. This is of great importance in establishing research problems which are suitable for investigation by a particular group of workers.

It should now be apparent that our discussion of intelligence has led to the conclusion that its *systematic* utilization, which we have found to be tantamount to problem solving, is synonymous with the research process, if the problems involved are properly chosen. It is to problem solving and problem classification that we now turn.

Problem Classification

All problems may be said to consist of those internal or external environmental situations in which an obstacle or gap in knowledge is interposed between the group or individual organism and an objective or goal. The problem solution consists in surmounting or circumventing the obstacle or filling in the gap, and achieving the objective. In other words, purposive behavior is required for resolving a problem. Reaching a solution will depend on the totality of the various interacting factors in the particular situation. These factors will, in turn, determine

¹⁵ Cf. Woodworth, *op. cit.*, pp. 778f.

some specific probability of success. Some of these elements have been discussed, and it may be noted that they are quite complex. A definite probability of success can be assigned to an individual or group for only the simplest problems. Nevertheless, it is this class of probabilities which is the determinant in establishing the efficiency of a research organization as discussed earlier. It is our objective to develop a methodology which will make it possible to increase the relative probabilities of success in solving problems in such organizations, although specific quantitative values of probable success may be indeterminable.

The problem situation is the inception of the research process. Without a problem there can be no research. "The scientific attitude . . . is necessarily alert for problems. . . . There is nothing which a scientific mind would more regret than reaching a condition in which there were no more problems. That state would be the death of science, not its perfected life."¹⁶ However, not all obstacle-goal circumstances make for research problems. An understanding of various types of problems which may be undertaken is essential in a study of this process. The specific problem situation will, to a large extent, define the approach which will have the highest probability of success.

Dewey classifies problem situations in terms of

The occurrence of a felt difficulty:

- a. In the lack of adaptation of means to end.
- b. In identifying the character of an object.
- c. In explaining an unexpected event.¹⁷

These appear to be the general classes of obstacles to which man devotes his creative abilities, and research problems may be found which fall within each of them. It may be assumed that the "gaps in the environmental situation" which we shall consider will be one of these types of problems. Thus, a great deal of industrial research comprises problems which involve the development of a process or product for achieving a specific purpose. In carrying out this research, much of the experimenta-

¹⁶ John Dewey, *The Quest for Certainty*, in *Intelligence in the Modern World: John Dewey's Philosophy*, Joseph Ratner, ed. (New York: Modern Library, Inc., 1939), p. 328.

¹⁷ Dewey, *How We Think*, p. 12.

tion is concerned with the identification of objects or substances, as well as attempts to explain unexpected results. At this point, let us accept the presupposition that all the problems we shall consider may be subsumed by this definition, and a more precise classification will be sought for our purposes.

Problems may also be classified as to the nature of the results which stem from their solutions. Thus, we might term a "practical" problem one which has as its primary objective a useful or necessary mechanism or process. That its solution could be an addition to the general store of knowledge should be apparent. A theoretical or mathematical problem, on the other hand, would be one whose solution would not immediately lead to a physical result. Many of industry's research problems fall within the former category, while the latter includes similarly a great portion of institutional research. Polya further divides these into "problems to prove" and "problems to find."¹⁸ Problems to find involve the discovery of a certain unknown and may include certain practical problems, or may be theoretical, abstract, concrete, serious problems or mere puzzles. He says, "The aim of a 'problem to prove' is to show conclusively that a certain clearly stated assertion is true, or else to show that it is false."

We see that Polya's classifications are based on the goals involved and are by no means mutually exclusive. Any differences in method used to attack these types of problems lie, in his opinion, in the nature of the knowledge required and not in one's attitude toward the problem. In other words, we may say that the class of problems of all types consists of subclasses which require specific combinations of intelligence for their resolution.

This is a significant delineation from the research standpoint, since it indicates that *the problems presented to any organization should be limited to those classes for whose solution no other types of knowledge are required than those available within the group*. Thus, the probability of success would not be high if we presented a problem in the theory of metal crystalline lattices to a group selected for, and experienced in, solving problems relating to the design of structural members.

Another classification of problems is that given by Northrop who considers problems of "logical consistency," of "empirical

¹⁸ Polya, *op. cit.*, pp. 136ff.

truth," and of "value." Problems of logical consistency are essentially *formal* "problems to prove" and require the use of a consistent system of logic for their solution. Problems of empirical truth involve the discovery of causal relationships in an external environmental situation and may require the use of formal logic for their full resolution. Problems of value involve the answering of questions as to what "ought to be," rather than what "is," the case. "It is the problem that designates the method, not the method which designates the problem."¹⁹ Thus, problems may be classified by the methods required for their solution. What these methods are, we shall discuss in the sequel. From the point of view of these definitions, most research consists of problems of empirical truth.

Problems may also be defined in terms of the complexity which they exhibit, requiring various classes of method for their solution. Warren Weaver has described problems of "simplicity," "disorganized complexity," and "organized complexity." Simple problems are those of only a few variables and relate particularly to mechanical problems in the physical sciences, such as were solved with such outstanding success in the seventeenth, eighteenth, and nineteenth centuries. Problems of disorganized complexity are those in which the solution must be obtained in terms of random-ordered sets by means of statistical techniques. "Indeed the whole question of evidence and the way in which knowledge can be inferred from evidence are now recognized to depend on these same statistical ideas, so that probability notions are essential to any theory of knowledge itself."²⁰

Problems of organized complexity are those in which the variables exhibit the essential feature of organization; *i.e.*, the variables studied are dependent upon individual and mass psychological effects, as well as purely physical ones. They are the "unknown middle ground" for present methods of problem solving. Many of the instances which might be categorized by Northrop's definition of value problems would fall into this category: life science questions, reproduction, economics, etc. Weaver feels that such problems can be attacked only by

¹⁹ F. S. C. Northrop, *The Logic of the Sciences and Humanities* (New York: The Macmillan Company, 1947), pp. 19f.

²⁰ Weaver, *op. cit.*, p. 542.

means of the broad scientific team. By their very definition, problems involving the broadest classification of the sciences are included within this category.

It should be noted that defining problems in terms of complexity includes, in part, the class of knowledge-defined problems (Polya), as well as the methodology (Northrop) classification. A careful definition of a given problem as to its complexity can furnish much information concerning probability of achieving a successful solution. Considering research problems, Weaver notes, "As an essential part of his characteristic procedure the scientist insists on precise definition of terms and clear characterization of his problem."²¹ To circumscribe research problems with a demand for clarity may be neither necessary nor desirable, as we shall see later. However, insistence on precision in definition can, of course, itself lead to suitable problems in research.

Another means of classification might be to consider the overall objective of an entire set of problems, as Churchman does when he says, "We feel that it requires no defense to say that one of the purposes of all scientific activity, *taken collectively*, is to reduce error to zero, *i.e.*, to become absolutely precise."²² This implies the division of all problems into a class in which the scientific method is applied in arriving at a solution, or into the negative of this class. As we have noted elsewhere, the scientific method plays a leading role in a systematic manner of problem solving. However, there is the possibility that some research problems could be satisfactorily solved by other methods. It seems preferable to outline a classification for problems which would depend on the type of obstacle to be overcome in achieving a solution and upon the type of solution obtainable, rather than upon the objective or methods utilized to solve it.

Problems require for their solution, (1) *a method or methods* (trial and error, insightful, scientific, statistical, etc.) and (2) *data* (simple, complex, sensory, imaginary, etc.). Answers are obtained by operating upon the data in terms of the method. From these requirements we may then outline four classes of problems: (1) those in which both data and method necessary

²¹ *Ibid.*, p. 542.

²² C. West Churchman, *Theory of Experimental Inference* (New York: The Macmillan Company, 1948), p. 173.

for solution are known; (2) those in which the necessary data, but not the method, are known; (3) those in which the method is known, but not the data; and (4) those in which neither the method nor the data are known.

In the first class, there may be further subclasses in which either the known data or method, or both, are undesirable. An example of such a situation was the case of Gauss described above, in which both the data and a method of solution were available to him. His objection to the method of solution available led to his discovery for himself of another more suitable one. A similar undesirability would lead also to subclasses under the second and third classes described.

These classes obviously include all those previously described, and based on this synthesis, the first of two restrictions distinguishing *research* problems from all other problems may be obtained. *Research problems include only those problems in which the method and data necessary to overcome the obstacle or gap and achieve the goal are not known simultaneously, or, being known, one or the other is not desirable.* Thus, a vast class of routine problems is eliminated from the research process. The research process then presupposes a search for the proper means or the requisite data, or both, in a particular problem.

The second restriction to be placed on problems falling into the research category has to do with reproducibility. A reproducible solution, in this sense, may be described as one which permits correct action to be based upon the results predicted by the answer. In any research work, some reproducibility is required if the answers obtained are to be at all profitable. It is too rigorous a restriction to require that only those problems whose solutions are *completely* reproducible be considered research problems. In certain types of problems of organized complexity, for example, it might never be possible to attain this objective, since the very process of obtaining a solution once might preclude the possibility of being able exactly to predict future results. However, we may require that *research problems shall include only those problems whose anticipated solutions are endowed with some implied measure of reproducibility greater than zero.* It is not required that this measure be known or even that it be a definite value above zero, but it should be possible to demonstrate that there exists some probability of re-

producibility greater than zero. This means that the questions asked or problems undertaken must be meaningful; *i.e.*, an answer must be actionable. In industry, to undertake a research project whose solution would have no meaning in terms of future activity would be manifestly absurd. This is not intended to imply that every problem must be known to be soluble in advance, but merely that if a solution is obtained, action may be taken upon the basis of the results.

To summarize, the research process involves systematic application of the ability to apprehend connections to a class of gaps in purposive environmental situations, in which the method and data necessary to achieve the goal are not simultaneously available or desirable. As Whitney notes,

The specific process of ordered reasoning comes out of recognized need to clear up a status of doubt and uncertainty, caused by a feeling that a blind alley or a blocked path confronts human experience. Scientific curiosity soon isolates and limits the problem dominating this cloudlike condition, and eventually makes the best guess for a solution in the light of all previous research.²³

The Reasoning Process

The mental mechanisms embodied in the reasoning process are probably equivalent at any given level of insightful behavior. Wertheimer's *Productive Thinking* outlines a dynamic theory of this mechanism based on the Pragnanz principle that the organization of a perceptual or conceptual field by an organism tends to be as simple and clear as the given conditions allow. This is sufficiently definitive to be useful in furthering our understanding of the manner in which the scientist resolves his research problems:

Dynamically the essentials in such thought processes seem to be briefly as follows: facing the problem, vectors arise in connection with and determined by the structural features, the gap, the incompleteness in the situation, tending to concretization of the trouble regions, and to the operations of change. Nothing in the place and direction of the vectors [*i.e.*, direction of the thought processes] is fortuitous. What is used, either out of the present situation or from recall, enters the proc-

²³ Whitney, *op. cit.*, p. 18.

ess by way of its function, as structurally required, changing the start-situation: a good transition from a bad gestalt [*i.e.*, the whole pattern] to a good gestalt.²⁴

Such a description implies an *early* recognition of the "gap" in an environmental situation. It is not essential that this recognition be completely clear and precisely formulated in the operation of the research process. Undoubtedly it will become clarified if a problem is resolved, but there are numerous examples of research problems whose solutions began with a hazy undefined idea that certain previously postulated relationships in a theory or practical technique were incorrect. Wertheimer describes the development of the theory of relativity, based on conversations with Einstein, in exactly such terms:

The process started in a way that was not very clear, and is, therefore, difficult to describe—in a certain state of being puzzled. First came such questions as: What if one were to run after a ray of light? What if one were riding on the beam? If one were to run after a ray of light as it travels, would its velocity thereby be decreased? If one were to run fast enough, would it no longer move at all? . . . To young Einstein this seemed strange.²⁵

The remainder of Wertheimer's description of the development of this theory follows his dynamic outline of the thought process. Clearly, the first step in research is the realization that there is a problem in attaining some desired goal. The goals or objectives are largely determined by the given total environmental and cultural situation, while the gaps or obstacles are defined by the action of these environmental factors and the goal upon a particular personality or group of personalities. The ability to visualize these gaps or "trouble spots" plus the ability to solve such problems define, in turn, the "creative mentality."

It is a postulate herein that the *creative mentality*, as defined in the above analysis, is a necessary requisite of the research process, and as a corollary, that *the ability to visualize gaps in a problem situation, and overcome them, is common to all such mentalities in any environmental reference frame*. Thus, we see that there is essentially no difference between the researcher in industry and in a university laboratory in any field of science.

²⁴ Wertheimer, *op. cit.*, pp. 52f.

²⁵ *Ibid.*, p. 169.

Such mentalities *must* form the core of all research organizations. Therefore, any increase in the efficiency of these mentalities should lead to an increase in the efficiency of a collective research activity.

From psychological studies such as Wertheimer's and the others mentioned, certain conceptual stages of the process have been formulated. It is generally agreed by most observers that there are four periods in creative activity: (1) indoctrination, study, analysis of the problem to the limit of the data or methods available, (2) recession, inattention to the problem, incubation, (3) enlightenment, insight, illumination—the solution, and (4) verification, elaboration, evaluation. There is no set time involved in any of these processes—seconds or years may be necessary in any one stage.

The period of incubation or recession has been the subject of much debate as to the exact mental process it represents, and whether it is conscious or unconscious. It is very possible that it is a period of learning, of setting up the data in the neuron-synapses circuits, much as is done in setting up a computing machine to solve a problem. However, there is no doubt as to the period of sudden enlightenment; as noted previously, it is described by almost all the research workers who have analyzed their own mental operations.

The ability and application of the creative mentality vary enormously from individual to individual. Many mentalities may work on a problem over long periods before it evolves from one stage to the next. It is the creation of an organization and environment to reinforce these individual abilities which will later concern us. These specific concepts are utilized in the discussion of research personnel in this study, in connection with specific research organization and administration.

Problem Goals

All organisms are confronted with desirable goals, and man is characterized by his ability to achieve relatively complex objectives. History records the continual utilization of man's creative ability to solve research problems and, certainly, this is only a fragment of the total span of the process. The problems determined by the cave man's environment, which he had to

solve in order to survive, were resolved by the same type of thought processes involved in the development of the theory of relativity. It should be emphasized here that we are considering only two parts of the research process: the problem and the creative mentality. The research problem and the creative mentality are postulated as necessary, but not sufficient, components of the research process. Combined with a systematic approach, or operational methodology, they form sufficient conditions. The lack of the necessary third component, systematic method, bars the cave man's activity, as well as much other human problem solving, from being defined as research.

We have said that it is the environment that determines the problems which man's creative mentality resolves. A brief historical sketch of the development of problems is sufficient to amplify this statement and to indicate the logico-historical consistency involved in the evolution of new problems from those previously solved. Primitive man was faced in his environment with immediate problems of survival in a world of physically superior organisms. It is highly likely that the earliest problem solving took the form of the development of protective implements, the first of which might have been the club. This development may have been fortuitous, as may that of the spear which probably followed the club as a weapon. On the other hand, the development of stone heads for spears, harpoons, sewing needles, etc., as evidenced by paleolithic artifacts, indicates the existence of the "flash of genius," stimulated by goals and the total environmental situation. The "invention" of the first needle, the *visualization* that these twigs or bones used to push thongs through animal skins would operate more effectively with the thong threaded through a hole in the end—surely this was the operation of creative mentality at an extremely high level in this environment.

Neolithic artifacts indicate further developments in the grinding and polishing of the tools and implements required for survival and the "invention" of the bow and arrow (again high creative ability). Each problem solved implied the application of further intelligence to the gaps created by that very solution. With an increase in security, resulting from these developments and others, of weapons, clothing, shelter, came problems of comfort and cultural pursuits. Explanations of natural phenomena

were undoubtedly created, religions evolved, drawing and painting invented. Of all these there is definite evidence extant.

Further evolution of the so-called "peaceful" arts gave rise to problems within the fields of building, agriculture, dyeing, etc. While many of these problems still concerned mere survival, there is clear evidence of a drive or urge on the part of man to solve problems because they trouble him, for the sake of the solving itself. Looms, saws, plows, pottery wheels, all these and more, were in existence in prehistoric times. These creations of man began to resemble machines as we know them, and, as Fraser notes,

These six simple machines [the inclined plane, the lever, the wedge, the screw, the wheel and axle, and the pulley] were all invented and used in prehistoric times. When Aristotle wrote his book on machines (350 B.C.) and entitled it *Mechanics*, these six were all well known from time immemorial and their origins were already veiled in the mists of tradition. The Greeks made no addition to the list. In fact, during the whole historic period up to the present, only one person has ever succeeded in inventing a new type of simple machine: in 1620 Pascal of Paris invented the hydraulic press.²⁶

As the environments change, so change the problems. The ones discussed appear to be common to all early societies, and advances beyond a certain point may not occur. Such advances depend on other environmental factors, such as communication, stimulation, climate, internal and external challenges, as well as the creation of a more general philosophic goal. If this philosophy is one of empirical acceptance of apprehended data as the only genuine knowledge, then causality is negated, and developments of the type we have been discussing cease. A civilization which accepts events with no curiosity as to causes or relations cannot be expected to develop actively in research. This was the general trend in oriental cultures. On the other hand, if an anthropocentric philosophy is evolved, as in the case of the Roman civilization, then the striving for comfort on the part of man will create numerous problems of day-to-day living. In each case, the creativeness of man will follow a different pattern.

The civilization of Greece, to which we owe the origin of so

²⁶ C. G. Fraser, *Half-hours with Great Scientists* (New York: Reinhold Publishing Corporation, 1948), p. 6.

many of our modern ideas, developed a great passion for ontological goals, seeking to discover a general explanation for life and natural events, "The Greek genius was philosophical, lucid, and logical. The men of this group were primarily asking philosophical questions. What is the substratum of nature? Is it fire, or earth, or water, or some combination of any two, or of all three? Or is it a mere flux, not reducible to some static material?"²⁷ These desires to understand the essence of being were created by the environment, and as a result of these goals, mathematical, natural, mechanical problems were uppermost. On the other hand, there was no great desire to predict and examine physical consequences, which made the Grecian theorists dependent largely on methods of observation, description, and classification. This is particularly true of Aristotle, and as we shall see, these tools are not completely sufficient for the true research method. Or as Whitehead says, "All this was excellent; it was genius; it was ideal preparatory work. But it was not science as we understand it." It should be emphasized that there is no apparent difference in the mental processes involved in the solution of the problems in the various civilizations described—the goals and the environment differ. Nevertheless, as we shall note again in our discussion of method, Grecian thinking served as "the second stage in the physical inquiry of Western science, for which the deductively formulated physics of Galilei and Newton is the third stage."

"It has been quite as important to the scientist to keep on finding questions as to keep on finding solutions."²⁸ And when those questions are formulated boldly and are part of a society of broadening scope, then equally broad and enlightening answers are obtained (we are not here concerned with the truth value of such answers), as the mathematical and philosophical works of the Golden Age of Greece exemplify. When the questions are concerned with the comfort and control of man himself, the answers are expressed in feats of engineering and codes of law, such as developed by the Roman civilization. The creation of a highly developed philosophy of eternal physical sur-

²⁷ A. N. Whitehead, *Science and the Modern World*, Mentor edition (New York: The New American Library of Literature, Inc., 1948), p. 7.

²⁸ H. T. Pledge, *Science since 1500* (New York: Philosophical Library, Inc., 1947), p. 21.

vival urged the Egyptians to the solution of prodigious problems of architecture and engineering, which in turn were closely linked with engineering problems which, of necessity, had to do with the flood periods of the Nile. The early Middle Ages in Europe were dominated by mysticism and questions of symbolism. Problems were raised and solutions entertained, but the problems, being symbolic in nature, were endowed with no "implications of reproducibility." In such an atmosphere, even improvements in the practical arts were unable to flourish. The environment diverted the creative mentalities, with a few exceptions such as Roger Bacon (*ca.* 1214–1292), into answering questions of little significance from the research point of view, and in an even less significant manner. Distances were great, communication was poor, and when significant questions were answered, the answers were lost, and the problems had to be solved again and again.

However, the Arabic culture was flourishing at this time, and questions were being posed and answered, which later were to affect the further development of Western civilization. And faith in an orderly nature, which was to play so predominant a part in the growth of the modern mind, was an indirect inheritance from the Medieval view of Aristotelian philosophy, that

... every detailed occurrence can be correlated with its antecedents in a perfectly definite manner, exemplifying general principles. Without this belief the incredible labours of scientists would be without hope. It is this instinctive conviction, vividly poised before the imagination, which is the motive power of research:—that there is a secret, a secret which can be unveiled.²⁹

With gradual changes in physical environment brought about by a multitude of diverse factors, and based upon a release from the domination of rationalistic symbolism, the philosophic goals of the environment turned to questions of the simple occurrences of life for their own sake.

Development of the Research Method

The focus of an entire civilization was changing. Trade was growing, towns were expanding, means were being found to aug-

²⁹ Whitehead, *op. cit.*, p. 13.

ment human labor with water and horsepower. The mines, the battlefields, the monasteries were all contributing their share of new problem solutions which were the keynote of this revival, this Renaissance.³⁰ The compass and the clock were invented or discovered, the one to give man a freedom of communication, the other a means of measurement in the temporal continuum, such as he had never enjoyed before. This is not to say that the times were becoming scientific or that research was flourishing—far from it—but the desire to dominate nature was emerging. And here magic played its important role.

Magic like pure fantasy was a shortcut to knowledge and power. . . . No one can put his finger on the place where magic became science, where empiricism became systematic experimentalism, where alchemy became chemistry, where astrology became astronomy, in short, where the need for immediate human results and gratifications ceased to leave its smudgy imprint. [However] . . . magic rested on demonstration rather than dialectic. More than anything else, perhaps, except painting [in which the extremely logically conceived concept of perspective was developed] it released European thought from the tyranny of the written text.³¹

It is not intended to catalogue the inventions and discoveries of these last fruitful five centuries but merely to indicate the growth and changing nature of the problems created by the environments. Printing was developed in the general environment of enlightenment in the fifteenth century. The need and desire for exploration and trade routes were influenced by, and to a large extent influenced, the development of astronomy and the heliocentric theory of the solar system in the following century. Problems suddenly seemed to appear on every side, and the *power* of knowledge was beginning to fascinate the creative mentalities of the time. Foundations were being laid in this antirationalistic philosophy for many of the problems of life sciences, of "organized complexity," etc., which are so profoundly affecting science today. The seventeenth century, "the

³⁰ L. Mumford, *Technics and Civilization* (New York: Harcourt, Brace and Company, Inc., 1934), pp. 13f. The clock was developed in the monasteries to toll the required canonical hours. "The clock, not the steam engine, is the key machine of the modern industrial age."

³¹ *Ibid.*, pp. 39f.

century of genius," was the synthesis of all these ferments and drew the pattern for the following generations.

A mere cataloguing of names, such as the following list of twelve given by Whitehead, is sufficient to indicate the depth and the fertility of the age: Francis Bacon, Harvey, Kepler, Galileo, Descartes, Pascal, Huyghens, Boyle, Newton, Locke, Spinoza, Leibniz. In this century, the stumbling block to true research was eliminated; systematic methods for arriving at answers to problems were developed. It will be sufficient to note with Mees that "There comes a point in technology [or science, or research], however, where progress is slow or even stops for lack of knowledge of the fundamental science [which is methodology when we speak of research as a whole]." ³² Now great thinkers were concerning themselves with the question: *What can we know and how can we know it?*

Petrarch, Boccaccio, Machiavelli, and Erasmus, far more than the alchemists must be considered the precursors of the modern scientific investigator. . . . But not only a few hardy skeptics . . . but also honest explorers and hardheaded statesmen and military commanders were the ancestors of all who endeavor to probe deeply to find new answers to old questions, who decide to minimize prejudice and examine facts impartially.³³

The new science was empirical, antirational if you will, but imbued with a deep faith that antecedents could be determined experimentally from consequences. It was the faith that was the heritage from Thomistic philosophy, plus the disregard for previously deduced theories not in accord with "stubborn irreducible" facts which were the keynotes of the new science. As is so often the case, the pendulum went too far and philosophy began to consider ultimate principles only upon a materialistic basis. Philosophy in science from the standpoint of individuality and subjective personality was scorned. For the solution of simple physical problems and the utilization of the concepts for the creation of machines which had "value in the market place," the seventeenth century viewpoint was ideal. The permeation

³² C. E. K. Mees, *The Path of Science* (New York: John Wiley & Sons, Inc., 1946), p. 43.

³³ J. B. Conant, *On Understanding Science* (New Haven: Yale University Press, 1947), p. 9.

of our culture with the ideas conceived during this time has continued to the present day.

The "perfect order" viewpoint was carried over into concepts of society in the following century. Apparently successful solutions of problems in both fields were marked by a mutual reinforcement, and although little new fundamental material was conceived, detailed development continued at an increasingly rapid rate. Lavoisier was laying the groundwork for modern chemistry, and the great mathematical physicists were doing the same in their fields (Laplace, Lagrange, Carnot, etc.). The creative mentalities in the fields of technology were utilizing some of the work of this and the previous century and with the general spirit of the times were achieving the so-called "industrial revolution." Textile machinery, steam power, large-scale iron and coal mining were all evolved and were solving and creating problems of their own by 1800.

At the same time the ubiquitous reaction was setting in. The creative artists were finding that "romanticism," the return to nature, offered at least a subjective solution to the human problems being created by the beliefs of scientific materialism. A reexamination of concepts was again under way. The irrational bifurcation required by the scientists, *i.e.*, that the career of an organism is completely determined by natural laws, and at the same time that the higher organisms can admit of purposive behavior, demanded a new synthesis for its resolution. This problem entered into science itself in the twentieth century, and it is not the intention here to do more than mention it as part of the environment which is setting problems for man.

In the nineteenth century the developments in technology reached a climax which changed almost completely the physical environment of Western civilization. The scientific methodology reached the creative mentalities in almost every field, and true research became a part of utilitarian development, much as it had in theoretical physics in the previous century. This was the century of steam and of engineering. Developments in the latter field paved the way for the more outstanding achievements in the creation of new fields of scientific thought which were to follow. Almost every new industrial development in this century laid bare problems in theory, and the development of new theories equally stimulated the solution of problems in technology.

As the steam engine defined problems for Carnot, so the theories of Maxwell called forth Siemens and Edison. Again, the changing environment was shaping the gaps, the obstacles, the overcoming of each in turn modifying the environment. The nineteenth century began to discover and resolve some problems relating to this very change—the theories of the conversion of energy and of evolution were prodigious achievements. It should be clear that the process of making a great stride forward is based on a multitude of false starts, or collection of many data, of many relatively minor achievements. ~ It is evolutionary in its own right.

This period was marked by the foundation and growth of scientific and technical institutions of learning, to supply the environmental need for mentalities trained in science. Schools of mines and polytechnology sprang up all over Europe. Scientific studies were added to the classical curricula in other institutions. The growth of industry was a prime factor in these developments, such schools in the United States as Rensselaer Polytechnic Institute (1825), Sheffield Scientific School (1846), Lawrence Scientific School at Harvard (1846), and others being formed primarily through grants from industrialists and merchants. The development of methods of teaching science was, and still is, an integral part of the research process.

In the twentieth century, the culmination of the work of the mathematical physicists has led to the posing of revolutionary problems of quantum theory and statistical mechanics, theories of relativity, and an attempt to reconcile advances in observed empirical data with a comprehensive theory. Along with a general reaction in society to the mechanistic and materialistic concepts, there has been a reexamination, within science itself, of the bases on which its philosophy is founded. This has been the age of organized research culminating in the great industrial empires and such developments as radar and the atomic bomb. The failure of science to solve problems of organized complexity, as well as the introduction of the multiplicity of problems of a highly organized industrial culture, leads us to the present day.

We have indicated that the creative mentality of any age is in close interrelationship with the total environment of his time and that, whereas the problems he solves are created by the interaction of the environment and his mentality, the solution of

a problem in turn creates new problems and new environments. Utilization of the creative mentality to solve problems whose data or method are not known simultaneously, and whose anticipated solutions have some implications of reproducibility, is the first requirement of the research process. The final requirement, the utilization of a systematic method or philosophy of solution, will now be considered.

CHAPTER III

METHODS OF PROBLEM SOLVING IN RESEARCH

Systematic versus Haphazard Methods

A method is an orderly process of procedure and, hence, the creative method may be considered synonymous with "application of intelligence in a systematic manner" as used in our earlier definition of research. This follows from the equivalence of "systematic" and "orderly" and from the discussion of the creative mentality and intelligence in the previous chapter. The process there described was not limited by any restrictions on method: research problems can be resolved by the mental processes defined as creative in a haphazard manner, by chance and accident. It was noted that such solutions were not to be considered part of the research process. This does not mean that fortuitous discoveries are not important in the evolution of science and knowledge, as well as research itself. On the contrary, it is freely acknowledged that a multitude of such happy accidents have, through the ages, endowed mankind with a great deal of significant information. Thus, Galvani noticed accidentally that muscle-nerve preparations of frogs' legs, hung from a copper hook, twitched when an iron support was contacted. His interest in this phenomenon led to Volta's work and paved the way for the invention of the electric battery.¹ However, the difficulties of analyzing what is presupposed to be a system-

¹ H. T. Pledge, *Science since 1500* (New York: Philosophical Library, Inc., 1947), p. 122. However, the interest taken by the individual in such accidental discoveries is also of prime importance. As Conant points out, Swammerdam had previously observed much the same phenomenon, though he never followed up his work. Galvani's interest made "subsequent discoveries inevitable." Clearly, an attitude of curiosity and a desire to explain unexpected events which occur in the pursuit of a problem are an important part of the creative mentality's role in research.

atic process, subject to certain rational rules of organization, are sufficiently complex, without the addition of the "disorganizing" factor of complete unpredictability. Therefore, chance discovery has been eliminated from this study of the research process, but not with a view to discounting its role in human progress.

The Evolution of Systematic Methods

The development of the systematic process for the solving of problems has been the task of philosophy, "the old and ever new undertaking of adjusting that body of traditions which constitute the actual mind of man to scientific tendencies and political aspirations which are novel and incompatible with received authorities."² Philosophy is the harmonization of the abstract with experience. The abstract in this case consists of certain critical methods of inferential judgment, logical systems of drawing conclusions in a given situation, or conceptual means of evaluating the relevancy of observations and testing the "truth" of hypotheses. In understanding research, it is necessary that we examine this background of method, since problems cannot be solved efficiently except through sound methodology. These methods are not themselves directly observable in nature. The philosophic task is to correlate the implications of consequences, resulting from the use of such means in resolving problems, with the observable environmental situation. It must thereby arrive at conclusions concerning the validity of the use of the method for predicting the future state of the natural phenomenon under consideration. "The work of philosophy as critical and constructive does not attempt to furnish additional knowledge beyond the reach of science. Its concern is rather with values and ends that known facts and principles should subserve."³ The fundamental problems of a philosophy of science may be, as Churchman has indicated, to answer the question, "What can be known, and how can it be known?"

² John Dewey, *Philosophy and Civilization, Intelligence in the Modern World: John Dewey's Philosophy*, Joseph Ratner, ed. (New York: Modern Library, Inc., 1939), p. 246.

³ John Dewey, "The Determination of Ultimate Values or Aims through Antecedent or *a priori* Speculation or through Pragmatic or Empirical

On the other hand, we are here concerned with the research process, and our definition indicates that it involves the solution of problems. Therefore, the important questions from the standpoint of this study are "What problems can be solved?" and "How can problems be solved?" It is not necessary to draw a precise distinction between these questions, but only to indicate that they are sufficiently synonymous so that a study of the development of the former is applicable to the latter.

Organization and administration of research require an understanding not only of the mental activity involved, which we have already discussed, but also of the various paths along which we may direct this activity in order to obtain satisfactory answers to problems. It is necessary, therefore, to develop the background of scientific method, which has provided us with the tools of logic which we can use today. The need for visualizing method as a developing tool for the research worker has been generally overlooked. The following development of this topic is intended to provide this background.

The problem of "knowing" implies the defining of "truth." If we say, "A is known to be B," this connotes that "A is B," that we may have stated an *ultimate* truth, a *relative* truth, a *stochastic* truth, an *empirical* truth, or whatever form of truth interests us. The nature of these "truths" and the manner of their determination have been considered by all philosophies, some of which have no relevancy to science or a scientific method at all. These latter need not concern us, since the solutions which they purport to offer for problems have no demonstrable measure of implied reproducibility. On the other hand, those philosophers who have examined the reflective process (which, it will be recalled, is considered herein one of problem solving) have formulated their theses in the following general terms: (1) *based on certain presuppositions*, (2) *certain classes of information may be abstracted by the human mind from its environment*, and (3) *from this information, conclusions can (or cannot) be inferred by*, (4) *following various predetermined operations in manipulating these observations*, (5) *which conclu-*

Inquiry," F. N. Freeman, *et al.*, *The Scientific Movement in Education* (National Society for the Study of Education, 1938), quoted in F. L. Whitney, *The Elements of Research*, rev. ed. (New York: Prentice-Hall, Inc., 1942), p. 6.

sions will lie within some class of truth values. This rather complex generalization covers any method used in research organizations to solve problems and, therefore, must be thoroughly understood if the process is to be directed properly.

The development of method has been dependent on the definitions and distinctions applied to each of these steps. The interpretations have been by no means identical, and it is also highly probable that what might be suitable for one class of problems would be most inefficient for another. There are certain differences between some of the methods reviewed and the research process. These will be found to be dependent principally on the definition of "truth value." For example, if we indicate that a particular method can undertake only to solve certain problems, leading to solutions of *absolute* truth for those,⁴ then we are not defining a method required of the research process. "Implications of reproducibility greater than zero" are not synonymous with ultimate truths or zero error. We shall be able to distinguish those methods appertaining to the class of problems which have been defined as comprising those of research and, ultimately, utilize these methods in synthesizing a rational pattern for the research process. This in turn should better fit us for improving the administration and organization of collective research activity.

The Early Philosophers

The devotion to a discovery of "method," which was to assume such overwhelming importance in the world after the Renaissance, is not discernible in the early great philosophers. As practical scientists, many of them undoubtedly had arrived at methods suitable for the solution of research problems; in their philosophies we find little to indicate that such methods were available or useful to test the complete explanations of nature which are so thoroughly postulated. For example, in Aristotle's *Mechanics* we find the following passage:

⁴C. W. Churchman, *Theory of Experimental Inference* (New York: The Macmillan Company, 1948), p. 49. He defines truth in terms of alternatives associated with the answering of a question, the truthful alternative being that one whose selection is associated with zero risk.

Why is it that dentists pull teeth more easily by applying the additional force of the tooth-puller than with the bare hand only? The truth is that the forceps consist of two levers, opposed to one another, with the same fulcrum at the point where the pincers join. Hence, they use the instrument for the extraction in order to loosen the teeth more easily.⁵

The explanation continues, describing the forces involved in terms of moments. Such mechanistic explanations, which were probably verified in experience, were common in the classical period. "Aristotle, on the other hand . . . was forced to reject [in his general philosophy] all postulated scientific objects [whose existence *might* have been experimentally verified] and to admit into science and philosophy only concepts by intuition."⁶ Since intuition excludes all inference or discursive reasoning, such a philosophy is at variance with the postulated concept of, for example, a fulcrum. Based upon the admission of intuitional concepts alone, manipulation of these concepts by deductive systems of logic to arrive at conclusions assumed to have all-embracing truth value gives rise to little or no implications of reproducibility. Therefore, such methodology is manifestly unsuited for the solution of research problems.

Creative mentalities have been resolving problems of various types since the genesis of man, but either the methods used have not been orderly or systematic, or the solutions arrived at were not reproducible, until the advent of experimental systems. The cave man in creating his sewing needle, and the Greeks in their cosmologies, are equally unsuitable as research prototypes. We shall consider briefly the philosophies of three great Grecian thinkers as illustrative of the close approach their systematic philosophies made to the scientific method and, paradoxically, of the great gap left unclosed by the lack of reproducibility in their solutions.⁷ An analysis of the philosophies of Democritus (ca. 460–357 B.C.), Plato (427–347 B.C.) and Aristotle (384–322 B.C.) will indicate the background of the creative environment

⁵ Aristotle, *Mechanics*, quoted by C. G. Fraser, *Half-hours with Great Scientists* (New York: Reinhold Publishing Corporation, 1948), p. 38.

⁶ F. S. C. Northrop, *The Logic of the Sciences and Humanities* (New York: The Macmillan Company, 1947), p. 88.

⁷ Or, in any event, in the manner in which their philosophies were transmitted to later ages.

transmitted to medieval times, which led eventually to the development of the research method in science and industry.

A fundamental principle of the later position is that of causality, and it will be seen that this natural assumption was held early by the Grecian philosophers. An examination of the reasoning involved in "cause and effect" methodology leads to the conclusion that it does not matter whether we postulate an *ultimate possibility of determining cause*, or merely *assume an orderly nature in achieving solutions to research problems*. A method used in research will be equally suitable in either case. Thales (*ca.* 640–546 B.C.) held that Nature "is not capricious nor subject to the whims of gods or goddesses, but governed by fixed and immutable natural law. . . . This fundamental tenet in science, he expressed in the famous saying: 'Necessity governs all.'"⁸

DEMOCRITUS

Democritus was an exponent of mechanistic explanation which is intimately associated with his atomistic theory of the universe. Nevertheless, he considered method to be secondary to a preconceived postulate of the nature of reality. It is not until much later that we find theories and hypotheses dependent upon testing their consequences. For example, the explanations of nature and man enunciated by Democritus are deduced from the following postulates:

1. From nothing comes nothing. Nothing that exists can be destroyed. All changes are due to the combination and separation of molecules.
2. Nothing happens by chance. Every occurrence has its cause, from which it follows by necessity.
3. The only existing things are the atoms and empty space; all else is mere opinion.
4. The atoms are infinite in number, and infinitely various in form; they strike together, and the lateral motions and whirlings which thus arise are the beginnings of worlds.
5. The varieties of all things depend upon the varieties of their atoms, in number, size, and aggregation.
6. The soul consists of fine, smooth, round atoms, like those of fire.

⁸ Fraser, *op. cit.*, p. 30.

These are the most mobile of all: they interpenetrate the whole body, and in their motions the phenomena of life arise.

This was a remarkably complete hypothesis, and in many respects is strikingly similar to some held today. It was capable of "explaining" much that was observable in nature, even though it decried such observations as mere "opinions." Lucretius (*ca.* 98–55 B.C.), in his *On the Nature of Things*, elaborated on this theory with examples such as a description of a violent storm illustrating that the invisible particles of air act in the same way as visible particles of water; perception of smells; the dampening of clothes on a moist day and their subsequent drying in the sun, with no visible interchange of water; the reduction in size of rings worn long on the finger, of stones acted on by water or by the feet, with no observable removal of matter. Though the theory required the concept of causality, it was not the cause and effect of experimental science. Every event had its causes, but there was no *necessary* implication that the same set of causes will give rise to the same event more than once. An environment conducive to the conception of such hypotheses was also favorable to the growth of certain types of intuitional mathematics, of which the Pythagorean school is an example.⁹

In summary, the Democritean theory was capable of explaining, in terms of its postulates, a large class of natural phenomena, makes no reference to proof other than a consistency with these postulates, and rejects sense perception as being mere opinion. Its influence on scientists of later ages was far-reaching, but alone its concepts were capable of solving few, if any, research problems.

PLATO

Plato was influenced by both the Democriteans and the Pythagoreans and probably came closer than any of the previous philosophers to a philosophy of science capable of being utilized

⁹ These were also founded on concepts based upon analogies to observed phenomena. Thus, Pythagoras is said to have been greatly impressed by the discovery that the concordance of a succession of notes on a lyre depends on certain proportions between the lengths of the strings on which they are produced. He applied the discovery to all sorts of things, including the "riddle of the universe." Cf. "Philosophy," *Encyclopaedia Britannica*, Vol. 17, 14th ed.

for the solution of problems of research. Utilizing all the science that was available to him and his disciples, he seriously examined questions of epistemology. He concluded that objects of sense perception were ceaselessly undergoing alteration and, therefore, that no prehension of ultimate truth was possible with respect to them. Real knowledge, on the other hand, he said consisted of *suprasensible* concepts, which he called "ideas" or "forms," and which are eternal and immutable. Quite possibly, the forms were intended to be principles or laws, to which the objects of the sensible world more or less conform.

And he attains to the purest knowledge of them who goes to each with the mind alone, not introducing or intruding in the act of thought, sight or any other sense together with reason, but with the very light of the mind in her own clearness searches into the very truth of each; he who has got rid, as far as he can, of eyes and ears, and so to speak, of the whole body, these being in his opinion distracting elements which when they infect the soul hinder her from acquiring truth and knowledge.¹⁰

The universe Plato describes is a teleological system, to be comprehended by reason alone, in which all things are adapted or subordinated to higher ends, these ends to other ends, and so on to the final end or purpose: *ultimate good*. For the understanding of this rational system Plato described certain fundamental rules of logical reasoning or dialectics. In the dialogues he indicated his insistence on "definition of terms, analysis of individual instances as a basis for generalization, the distinction between opinion or wishful thinking and knowledge, and between words and things." He advocates the study of unified science, that students should attempt "by a method of questioning and criticism, analysis and synthesis, to rise to a grasp of general principles and to see the relation of each branch of knowledge to the rest."¹¹

Such clearly enunciated principles are suitable indeed to the solution of research problems, and *by chance* the solutions could even attain implications of reproducibility. These solutions were completely discursive, their merits depending on the com-

¹⁰ Plato, *Five Great Dialogues*, trans. B. Jowett (New York: Walter J. Black, Inc., 1942), p. 95.

¹¹ *Ibid.*, pp. 25ff., 419.

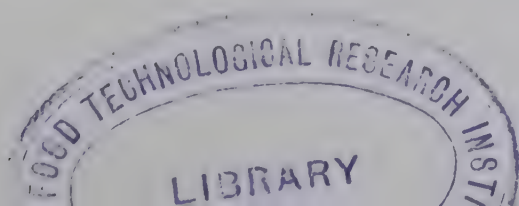
pleteness of classification and subdivision of relations, to reach answers to questions by reasoning alone. If observations did not clearly agree with the rationalities disclosed by reasoning, it is implied that the cause is the imperfection of the "object-world" as a copy of the world of "ideas," of "reality." *Physical* tests of the veracity of the reasoning were generally precluded by the presuppositions of the philosophy.

Again, we find ourselves at a loss to solve research problems with some consistent implied measure of reproducibility. The difficulty of achieving any consequential results in research in which answers are not tested by action in an actual situation should be readily apparent. It is not sufficient to deny the reality or perfection of the physical world and thereby obtain solutions to research problems which will be consistent within our systematics. In order to transcend this condition which we have imposed upon the research process, it is necessary that some systems of measurement be provided, which may not even approximate identity for all observers but which will have some recognizable features of coincidence. The creative environment provided by Platonic philosophy was *almost* sufficient, and much of the needed information was at hand. Nonetheless, the lack of recognition of the necessity for some replicative means of physically examining logical consequences proved an insurmountable obstacle to any but slow and unsure advances in science. And, although Plato certainly did not intend it to be so, the very nature of his reasoning was such as to inculcate a continuing respect for *authority* among those who followed in later centuries.

ARISTOTLE

The philosophy of Aristotle, who was a disciple, researcher, and teacher for some 20 years under Plato, can truly be said to be an enunciation of a scientific method. This might have been anticipated as a result of the interaction of the Platonic environment on such a great mind. The loss of his works to the Western world for centuries and their subsequent *authoritarian* disinterment served as a bar to an effective use of his methodology.¹² In

¹² This is an extreme example of the consequences of the lack of effective means of communication of scientific material, which at this stage



his thought we see for the first time a recognition of the necessity for the *experimental* verification of theory, and an understanding that theories which failed the test of reproducibility were inadequate. His early work followed the Platonic philosophy, but gradually he began to recognize the difficulties inherent in solving scientific problems when objects of sense perception are eliminated from method, and the appeal is to reason alone. He was both a great thinker and a great experimental scientist. In his *Organon* or *Logic*, he describes his conception of the processes of systematic reasoning and verification of its conclusions. As his starting point he utilizes *intuitively conceived* fundamental assumptions or axioms.

Every science, Aristotle points out, must begin with a few of these necessary axioms or general truths. They cannot be logically proved, but our minds by simple intuition accept them nevertheless as obviously true to actual fact. Without some such assumptions as foundation stones we could never start to build anything. . . .

Beginning then in each case with certain assumptions that are to the holders so evident that they can dispense with logical proof, men proceed to construct their systems of knowledge and science. They add to what they already know something more that is both new and sure . . . by deductive reasoning, from previously established general rules down to particular cases. . . .

In all syllogistic or deductive reasoning, however, Aristotle warns us, we must make sure that the first or basic proposition is really comprehensive and covers every case.

A second form of reasoning that may lead to valid conclusions . . . is what we call inductive argument, establishing, that is, a general rule on the evidence of many single, observed facts or instances. . . . Here, however, Aristotle reminds us, we must be constantly on guard against drawing conclusions too hastily. Unless the number of instances on which we ground our generalization is large enough to be thoroughly representative, there may be instances we have overlooked to which it does not apply.¹³

Here is a *rational* methodology indeed, and one which we shall find not greatly different from those in use today. And, as for

may have been the most serious bar to rapid development. Cf. C. E. K. Mees, *The Path of Science* (New York: John Wiley & Sons, Inc., 1946), p. 69.

¹³ Aristotle, *On Man in the Universe* (New York: Walter J. Black, Inc., 1943), pp. xviii ff.

implications of reproducibility, Aristotle recognized their importance in science when he said, discussing his own conclusions,

. . . the actual facts are not sufficiently made out. Should further science ever discover them, we must yield to their guidance rather than to that of theory; for theories must be abandoned, unless their teachings tally with the undisputable results of observation.¹⁴

It is clear that Aristotle was laying the foundations of the research process, providing means whereby problems could be solved by the development of method, and observation of data, and indicating the necessity for some measure of reproducibility in the solutions. Unfortunately, the authoritarian environment of learning, encouraged in part by his own writings, his followers, and the aura of his greatness, prevented further advance for centuries. It may be, as Mees says of those who followed, that "Their actual progress in physics was certainly much handicapped by their feeling that practical experimental work was not suitable for a philosopher and thinker."¹⁵ There are those who have criticized Aristotle for being too much of an empiricist, but we would agree with Ratner that they are

. . . making Aristotle shoulder the blame for the benighted centuries that succeeded the downfall of Greece. By putting his refined objects of reflection in *rerum Natura*, Aristotle put them where they could be empirically got at and tested. That they were not empirically tested before Darwin is no fault of Aristotle's.¹⁶

Nevertheless, we find that Aristotle did not seem to recognize the *experiment* as such—his insistence on the conformity of theory with fact was based on observation, description, and classification. There is no indication that he visualized the control of natural events or observations to increase the predictive power of the solutions to the problems of his day. The implication (an inheritance from Platonic philosophy) was that, given sufficient observation, and proper reasoning therefrom, theories could be developed which could explain all natural phenomena. It is probable that this lack of any comprehension of the necessity for experiment was the most important deficiency in his method, and

¹⁴ *Ibid.*, p. xvii.

¹⁵ Mees, *op. cit.*, p. 68.

¹⁶ Ratner, ed., *op. cit.*, "Introduction," pp. 210f.

as we shall see, the rapid growth of the body of scientific knowledge began with the emergence of the experimental concept. This recognition of experiment simply was not a part of the philosophic environment of the time, but the contributions of Aristotle should not be deprecated on this account. Northrop points out, as we have noted elsewhere:

In fact Aristotelian physics was the second stage in the physical inquiry of Western science, for which the deductively formulated physics of Galilei and Newton is the third stage.

From our standpoint, it is true that this philosophy was capable of solving certain research problems—those in which this logical method is applicable and in which satisfactory data are obtainable by noncontrolled observations. These are capable of being solved with some implied reproducibility. Aristotle and his contemporaries did solve such problems,¹⁷ for example, the theory of moments mentioned previously. However, the methods which he outlined, and which are definitely a part of the present-day apparatus of problem solving, were confined to intuitionally conceived concepts, reinforced by empirical observations. These were deductively or inductively formulated into particular solutions or general theory. *The data were confined to those subject to variance beyond the control or measurement of the observers.* For the latter reason particularly, the structure lacked the flexibility which we shall find is essential in an efficient research process. However, a great deal of research is now conducted with no reference to more efficient methods, although the background of scientific knowledge permits the attainment of more reproducible answers. Thus, experimentation which does not utilize modern statistical techniques in analysis of variances, say, and thereby increases the amount of time required to obtain the desired solution is undoubtedly inefficient.

Aristotelian science was able to erect a body of knowledge which “explained” the relations of a great many phenomena. For example, Aristotle explained the ascent of water in a pump by nature’s *abhorrence* of a vacuum, which served satisfactorily

¹⁷ Mees, *op. cit.*, p. 68: “Instead of developing experimental science, the most popular Greek philosophers based their views of nature on *a priori* assumptions, and their progress was largely confined to pure mathematics, especially geometry and the theory of numbers.”

until it was necessary to solve the problem of the observable limitation on pumping height. Then such experiments as Torricelli's and Pascal's, the latter arranged an experimental ascent of the Puy-de-Dôme with a barometer which had been invented by the former,¹⁸ offered more efficient results. In the first instance, the postulated explanation was no explanation at all in terms of physical realities or operational tests. On the other hand, the second explanation was capable of being tested *and utilized* to take action in specific instances.

In any event, with Aristotle the scientific method had reached a peak from which it was to recede for centuries following. At this point, the creative environment comprehended the recognition of the following ingredients of the research method: (1) *the necessary assumption of cause and effect*, (2) *the requirement of basic intuitional postulates*, (3) *the desirability of observation, classification, and description*, (4) *the possibility of either deductively or inductively formulated solutions*, and an (5) *implied recognition of the necessity of reproducibility and consistency with "stubborn, irreducible facts."* The formulation is not yet complete, nor applicable to all classes of research problems, as we have seen. We shall return to a more complete examination of each of these concepts when all the elements have been historically defined.

Post-Hellenic and Medieval Method

After Aristotle's death his Peripatetic School in Athens apparently lost his library and treatises, and,

. . . forgetting his spirit, . . . [it] rapidly hardened into a logical tradition of its own; and even when the treatises were recovered, they were treated not as incentives to enquiry and further discovery but as a rounded body of complete knowledge (perhaps the last thing that

¹⁸ J. B. Conant, *On Understanding Science* (New Haven: Yale University Press, 1947), pp. 29ff. The development of this problem is carefully discussed. Reference is made to Galileo who modified the Aristotelian concept by postulating that the water column would no longer support its own weight above a certain height (by analogy with a metal wire—this was not an unreasonable concept prior to the availability of necessary experimental data). We shall see later that problem solving by analogy is a valid procedure for *research*.

Aristotle would have claimed for his tentative conclusions), on which commentators might write and lecture as if it contained the final word of perfection. . . . In this way the great researcher was made the enemy of research . . . and thus the beginning of modern science in the 16th century took the form of a revolt against Aristotle—one of the most scientific spirits that ever lived.¹⁹

After Aristotle, classical philosophy turned mainly to eclecticism and ethics. The reflective, critical nature of the earlier philosophies was lost, and the philosophic environment consisted of moral and religious problems, rather than problems of understanding nature. The stoics attached no intrinsic importance to knowledge, and the skeptics renounced all claim to it. Obviously, methods developed under these types of philosophies could neither discern nor satisfactorily solve research problems. The loss of Aristotle's teachings to the Western world led to the revival of Platonistic thinking, of which Augustine (A.D. 354–430) was a leading exponent.

Augustine, who originally was a skeptic, constructed a Neoplatonistic philosophy based on the indubitable experience of doubting. As did Plato, he considered that the only true realities were concepts constructed without reference to sensory data. Descartes later advocated methods of science constructed along similar lines. These intuitional truths were apprehended by the "immortal soul" and are created, not by the mind of man, but by God. This theological philosophy reached its climax with Augustine. From the standpoint of scientific method, it should be apparent that the very recognition of problems of research implies a certain *attitude of skepticism or doubting*. Therefore, its use occupies a place in our development of the methodological pattern. However, the intellectual life of the several centuries following Aristotle was generally uncreative, and little or no methodological advances were made in this mental climate.

With the passage of time, some of Aristotle's studies of logic became known, and by the tenth century, some of the philosophic problems which had been kept alive by the Neoplatonists were being sporadically considered in the light of these methods. The growth of scholasticism, which dominated the thought of the Middle Ages, had begun. The reconciliation of Christian

¹⁹ "Aristotle," *Encyclopaedia Britannica*, Vol. 2, 14th ed.

theology with philosophy was the chief concern of those mentalities which might, in other environments, have been scientifically creative. This movement culminated in the teachings of Thomas Aquinas (1225–1274). The Crusades played a large part in the revival of Aristotelian thought (as well as the *introduction* to the West of that of the Arabs) by the recovery of many manuscripts which had been preserved in Eastern lands.

The major components of method inherited from this period were further developments in deductive logic and its requirements for definiteness in thought, as well as the sense of complete causality which we have noted previously. As always, the process of change was under way, and although the rate was slow, there were definite signs of reaction to the methods so firmly entrenched. The knowledge of the Arabs, who appear to have been among the greatest of the eclectics and assimilators, was being spread. The Arab interest in mathematics and medical knowledge, which the scholastics had cast aside, and their bent for patient experimentation, which the Greeks had disdained, were to have a major influence on the further development of method.

ROGER BACON

The inevitable product of these changes in the environment on the creative mentalities of the time is perhaps best illustrated by Roger Bacon, who was probably more original in his understanding of the difficulties of scholasticism and in his criticism of the methodology than in his constructive contributions to knowledge. He considered himself an outstanding interpreter of Aristotle, and certainly no rebel against the church, although he suffered imprisonment for some fifteen years because of his writings. Nevertheless, he illustrates the growing position of inquiries as to more fruitful methods than dialecticism for obtaining solutions of problems. He pointed out that the four great causes of error in reasoning are “(a) authority, (b) custom, (c) the opinion of the unskilled many, and (d) concealment of real ignorance with the pretense of knowledge.”²⁰ This is certainly in the Aristotelian and Platonic spirit. In addition to this, he also wrote:

²⁰ T. L. Kelley, *Scientific Method* (Columbus, Ohio: The Ohio State University Press, 1929), p. 163.

Experimental Science has one great prerogative in respect to all other sciences, that *it investigates their conclusions by experience*. For the principles of the other sciences may be known by experience, but the conclusions are drawn from these principles by way of argument. If they require particular and complete knowledge of those conclusions, the aid of this science must be called in. It is true that mathematics possesses useful experience with regard to its own problems of figure and number, which apply to all the sciences and experience itself, for no science can be known without mathematics. But if we wish to have complete and thoroughly verified knowledge, *we must proceed by the methods of experimental science*.²¹

Our choice of a few outstanding figures illustrating the development of methodological thought and the brevity of this discussion should not be interpreted as signifying any distinct discreteness in the evolution of ideas. No doubt there were numerous other thinkers and philosophers who contributed to the unfolding of the pattern. We are not attempting here a history, but rather a logico-historical analysis of the major components of the scientific method, and examples have been selected with this view. The stage has now been set—the philosophic background is complete for the “creators” of the scientific methodology in the seventeenth century.

Countercurrents and reactionary diversions were extant at all times, even as they are today. Thus, the Renaissance, which was so important in many respects as we have seen, gave rise to humanism and an antiscientific bias. Although humanism was most important in the battle against scholasticism, it also argued strongly against investigations in natural science, urging a study of man alone. This type of antischolastic and, at the same time, unscientific thought is well illustrated by Erasmus' statement that “. . . so are they most happy of all others that have least commerce with Sciences, and follow the guidance of Nature, who is in no wise imperfect, unless perhaps we endeavor to leap over those bounds she has appointed to us.”²² Whether such philosophy, as Randall indicates,²³ set back scientific development

²¹ Roger Bacon, *Opus Maius*, Bridges, ed. (New York: Oxford University Press, 1897), Vol. 2, pp. 172–173. Italics added.

²² Desiderius Erasmus, *The Praise of Folly* (New York: Walter J. Black, Inc., 1942), pp. 149ff.

²³ J. H. Randall, *The Making of the Modern Mind* (Boston: Houghton Mifflin Company, 1926), p. 212.

some three hundred years is not important to our purposes. It is our thesis that problems are created by the interaction of the entire environment with a particular mentality or group of mentalities, and that in methodology, as well as in the physical world, action is synonymous with reaction. Therefore, we may reasonably conclude that *all these streams of thought we have mentioned were prerequisite to the flowering of the scientific method.*

The Emergence of Research Methods

This flux of thought, this reaction against authoritarianism, this questioning of accepted solutions of natural problems, as well as similar and simultaneous developments in everyday life, burst forth in the sixteenth and seventeenth centuries in an overpowering quest for methods of discovering "truth." The great scientists of the time, and others who were not discoverers, formulated methods of discovery and verification. The experiment was recognized as a fundamental component of this process, and numerous scientists and artist-engineers were writing and lecturing on its use. The method of Galileo will serve as an excellent example of the path the new philosophy was following.

GALILEO

Galileo's discoveries are well known; his work founded the science of mechanics and paved the way for Newton. The method which he utilized and developed comprised the combination of experiment with calculation, of analytical and synthetic methods. He stressed the transformation of the concrete experimental results into abstract mathematical terms (as had Leonardo da Vinci before him) and the assiduous comparison of results. In essence, he would select a single instance, like that of a ball rolling down an inclined plane, experimentally determine the reproducible elements of the instance, analyze these elements to determine mathematical relationships, accept those relationships as a working hypothesis or principle, deduce further consequences mathematically, and experimentally test these consequences for verification of the principle. Here indeed was a method of physical science, one fully capable of solving research

problems as we have defined them, with demonstrable measures of reproducibility.

In the more general terms outlined previously, Galileo's method may be described as follows: (1) Based on the presuppositions that the objects of sense perception are real and embody causality, that the errors of spatial and temporal measurement are of a sufficiently small order of magnitude, and that the variables relevant to the particular event involved can be intuitively apprehended, (2) certain comparable physical measurements of these variables may be made, abstracted from the environment, during the course of an experiment, (3) from these measurements certain relationships can be inferred, (4) by symbolic manipulation (mathematically), consequential conclusions can be deduced from these relationships and tested by further experiments of a similar nature, and, if verified, (5) the relationships and conclusions will be implicitly true.

We shall see later that this method contains an inherent fallacy, known as "affirming the consequent," but for the present we may note that this has largely been the method of the physical sciences to the present day. Despite this clear enunciation and use of a method suitable for solving research problems, Descartes and Francis Bacon were being accorded more recognition at the time in the evolution of scientific methodology. It is to an examination of their doctrines which we now turn. The contribution of Galileo will be reconsidered below in the light of the entire seventeenth century's additions to the philosophy of science.

FRANCIS BACON

Francis Bacon described himself as the herald of modern thought and the developer of the new experimental method. As far as his own times were concerned, he was more successful as a dispeller of scholastic attitudes than as a harbinger of new methods. Nonetheless, he recognized the investigation of nature as the most important approach to the solution of problems, saying:

The signs for the interpretation of nature comprehend two divisions: the first regards the eliciting or creating the axioms from experiment, the second the deducing or deriving of new experiments from axioms.

. . . For we must first prepare as a foundation for the whole a complete and accurate natural and experimental history. We must not imagine or invent, but discover the acts and properties of nature. . . . And this collection must be made as a mere history, and without any premature reflection, or too great degree of refinement.²⁴

His description of scientific method is not patently different from that of Aristotle, although he visualized himself as anti-Aristotelian, basing his contentions on the contemporary conceptions of classical philosophy. But he did recognize the necessity for patient exploration and laboratory experiment, notwithstanding the difficulties his system would place in the path of the would-be discoverer. He advocated the casting out of all preconceived notions or "idols," and the painstaking collection of data, from which he felt general principles could be induced, and plans laid for further experimentation and collection of additional data. The data collected in any field were to be collated into three classes or tables, based on the *absence*, *presence*, and *comparison* of observed "qualities." "He had in his mind the belief that with a sufficient care in the collection of instances the general law would stand out of itself."²⁵ His philosophy tells us that (1) based only on the presupposition that observed events are real and embody causality, (2) information may be collected from the environment by observation of natural phenomena and experiment, (3) this information may be classified as to agreement, difference, and comparableness, and (4) from this classification inferences may be drawn as to relationships which will have general validity.

This doctrine was developed further by his successors, culminating in Mill's system of logic. Answers to certain research questions are obtainable by means of this method, particularly those in which data are completely lacking, and in which there are few signposts to guide the observer in formulating the method of a particular experiment. However, it certainly was not the method of the physical sciences of Bacon's time, and subsequently, induction has turned out to be much more complex a

²⁴ Francis Bacon, *Novum Organum*, Basil Montague, ed. (Philadelphia: Carey & Hunt, 1844), quoted by H. Boynton, *The Beginnings of Modern Science* (New York: Walter J. Black, Inc., 1948), pp. 589f.

²⁵ A. N. Whitehead, *Science and the Modern World*, Mentor edition (New York: The New American Library of Literature, Inc., 1948), p. 44.

process than he had envisaged. From the standpoint of a complete system of inquiry, the Baconian approach comprehends a single stage—in Northrop's terms, the second stage of logical inquiry is “. . . the Baconian inductive observation of the relevant facts to which the analysis of the problem leads one.”²⁶

Bacon's major contributions to the methodology of research are (1) *his insistence on the casting aside of all previously formulated solutions*, (2) *the requirement of painstaking experiment for the formulation of successively greater generalizations from observed facts*, and (3) *the visualization of the use of controlled conditions in laboratories by collective groups of researchers*. His greatest failure was in his insistence on the first of these concepts; it can be a useful tool in some instances, but certainly not in all. It undoubtedly stemmed both from the conditions of the age and Bacon's own disregard for the power of mathematics. His concept of collective scientific endeavor was influential in the formation of the Royal Society. His concept of useful scientific knowledge as enabling man to attain power over nature is still the keynote of the greatest part of research activity.

DESCARTES

Descartes, on the other hand, was primarily a mathematician, a geometer, who felt that the state of knowledge under scholasticism was due to a lack of mathematical method. As noted previously, he adopted from Augustine the instrument of methodical doubt, rejecting everything that was open to doubt until he could find something indubitable. His procedure was purely rationalistic—from the minimum intellectual concepts remaining after all has been tested by doubting, the remainder of all knowledge may be deduced. Clarity, he felt, was the best test of knowledge: “Whatever I apprehend very clearly and distinctly is true.”²⁷ His concepts of scientific method are clearly opposed to those of Bacon, although he accepted the utility of experiment and observation in clarifying the indubitable concepts. He wrote,

²⁶ Northrop, *op. cit.*, p. 29.

²⁷ René Descartes, *Meditation III*, quoted in “Philosophy,” *Encyclopaedia Britannica*, Vol. 17, 14th ed.

That in order to see truth, it is necessary once in the course of our life to doubt, as far as possible, of all things. . . . That we ought also to consider as false all that is doubtful. . . . [however] That we ought not meanwhile to make use of doubt in the conduct of life . . . [since] the opportunity of acting would not infrequently pass away before we could free ourselves from our doubts. . . .

That we cannot doubt of our existence while we doubt, and that this is the first knowledge we acquire when we philosophize in order. . . . Accordingly, the knowledge, *I think, therefore I am*, is the first and most certain that occurs to one who philosophizes in an orderly way. . . .

That we never err unless when we judge of something which we do not sufficiently apprehend. . . . But the reason why we are usually deceived, is that we judge without possessing an exact knowledge of that which we judge. . . . The chief cause of our errors is to be found in the prejudices of our childhood. . . .

Wherefore, if we would . . . give ourselves to the search for all the truths we are capable of knowing, we must, in the first place lay aside our prejudices, . . . in the next place, make an orderly review of the notions we have in our minds, and hold as true all and only those we have clearly and distinctly apprehended. . . . Besides the notions we [will then] have of God and of our own mind, we will likewise find that we possess the knowledge of many propositions which are eternally true. . . . We will further discover in our minds the knowledge of a corporeal or extended nature, that can be moved, divided, etc., and also of certain sensations that affect us, as of pain, colors, tastes, etc., although we do not yet know the cause of our being so affected. And, comparing what we have now learned, . . . we shall acquire the habit of forming clear and distinct conceptions of all the objects we are capable of knowing. In these few precepts seem to me to be comprised the most general and important principles of human knowledge.²⁸

This philosophy presupposes that clear and distinct principles and conceptions of objects are real. These may be abstracted from the environment by observation by the process of doubting all, leaving only those acceptable to reason. Further consequential relationships may be deduced from this irreducible minimum, which deductive inferences will be valid as a conceptual scheme of "truth." In the light of our previous development of classical methodology, we find Descartes had formalized

²⁸ René Descartes, *Discourse on Method*, trans. John Veitch (Everyman's edition; New York: E. P. Dutton & Co., Inc.), quoted in Boynton, *op. cit.*, pp. 595ff.

(but not newly constructed), utilizing a mathematical model, (1) *the advisability of skepticism* (as far as prejudices were concerned), (2) *the necessity of formulating basic intuitional postulates*, and (3) *the utilization of deductive logic*.

This method also is capable of solving some research problems, as we have defined them, but it would not be efficient to attempt to use it to solve all problems of research. If we were concerned with the implications of research as regards "truth value," we might consider this method of deductively formulated theory as another stage in the entire process of scientific inquiry. The problems particularly susceptible of resolution by these concepts are those in which the data are capable of being symbolized, or reduced by abstraction to symbolic terms, so that they may thereby be subjected to the operations of deductive logic. As we shall see, it is dangerous to assume that any one method is extraordinarily capable of endowing its solutions with measures of reproducibility. Actually, the Cartesian philosophy of a reason outside of human experience governing and constituting the order of nature, if utilized solely, would pose more difficulties than it would overcome for the research worker.

NEWTON

Newton, being more of a practical scientist than either Bacon or Descartes, synthesized and integrated the elements of both their methods which were capable of solving the problems in which he was interested. His methodology, which was similar to, but broader than, Galileo's, was more than likely an a posteriori rationalization from his successful scientific work. He included (1) the observation of events, (2) the induction therefrom of a general principle, (3) the deduction of consequences mathematically from this principle, and (4) further experiment and observation to indicate agreement of experience with the induced principle. His rules of reasoning in philosophy, from the *Principia*, are perhaps the apotheosis of the strictly mechanistic approach to problem solving:

Rule I. We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. . . .

Rule II. Therefore to the same natural effects we must, as far as possible, assign the same causes. . . .

Rule III. The qualities of bodies which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.

Rule IV. In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions.²⁹

These were the principles of method, mathematically inspired but admitting of physical experiment, which were eminently suited to the solution of problems of simple variables. This fully grown pattern for scientific research traced the development of its fundamental concepts from preclassical thought. The question of knowledge and the world of real objects vs. the world of the reason had been solved as far as science was concerned. The materialistic philosophy which then was developed from the mechanistic scientific method, by Hobbes and others, was completely satisfactory for the problems of physical science which were so rapidly solved in the eighteenth century. Do we then have a sufficient methodology for our research process? The answer lies in the fact that this method became sufficiently static, as far as science was concerned, so that it was inefficient when the environment gave rise to new types of problems, such as those of organized and disorganized complexity.

Critical Studies

"The eighteenth century aspiration to resolve the world in terms of a mathematical-mechanical synthesis was to prove over simple."³⁰ Nevertheless, experiment was being extended on every hand, and with each new development, concurrently came new problems, so that the desired end was never reached. Instruments were being developed and reinforcing the general

²⁹ Isaac Newton, *Principia Mathematica*, Florian Cajori, ed. (Berkeley: University of California Press, 1934), quoted in Boynton, *op. cit.*, pp. 604ff.

³⁰ H. L. Whiteway, *Scientific Method and the Conditions of Social Intelligence* (St. John's, Newfoundland: Trade Printers and Publishers, Ltd., 1943), p. 79.

tendency toward the experimental method. The inevitable reaction was setting in, and men were beginning to examine the *philosophic* truth values of the results of the mechanistic explanation of nature. The acceptance of fundamental intuitions, and experimentally demonstrated deductions from them, as absolute truth was again raising questions of what can be known. The questioning of the limits of knowledge by Bishop Berkeley, the skepticism of Hume, and the criticism of empiricism by Kant had little effect on the scientific methodology of the time. However, these were precursors of the more modern views of the provisional character of science, including the relativists and pragmatists, to whom all truths are relative and to whom answers to problems "at best serve relatively immediate ends."³¹ As far as the method of science was concerned, this was the beginning of the realization that the closed systems of the seventeenth century were inadequate to answer questions which arose from the expansion of knowledge, and whose resolution could not verifiably be obtained on the bases of the thus far accepted primary intuitions and concepts. A place was being reserved in the methodological pattern for statistical approaches, indeterminacy, and modern experimentalism.

Evolution and Method

The mechanistic explanation of natural phenomena soon spread to the zoological and anthropological fields, and the doctrine of evolution was developed. The Lamarckian and Darwinian tenets involved no new methods of science, being in part based on the Baconian inductive approach. From some viewpoints, the genesis of this theory can be said to be the crowning achievement of the inductive method. While the descriptive and classificatory approach of the biological sciences had been previously founded by such observers as Linnaeus and Cuvier upon the doctrine of special creation, the careful collection of instances implying evolution by Darwin was based upon a new, if tentative, hypothesis.

Nature, it has been said, gives no reply to a general inquiry—she must be interrogated by questions which already contain the answer she is

³¹ Churchman, *op. cit.*, p. 165.

to give; in other words, the observer can only observe that which he is led by hypothesis to look for; the experimenter can only obtain the results which his experiment is designed to obtain.³²

Even here, pure collection of observations and instances would have accomplished little. The necessity for intuitional postulates on which to base experiment should be apparent. Every research worker must make decisions in advance of experiment as to *what* he is going to measure, *how* he is going to measure it, and *how* he intends to manipulate his measurements so as to answer the questions, however vague, he has posed for himself.

Biological evolution reacted upon physical science in such a way as to indicate the utility of *another* methodological concept in problem solving. If evolution is recognized as a useful tool in biology, can it not serve similarly in the study of a physical problem? Is not the development of a theory or answer to a recurring problem subject to an equivalent development, and is it not possible to draw some profitable conclusions from a study of such an evolution? This approach has some merit and is utilizable in the solution of problems concerning which data have been collected for a sufficient period of time so that solutions have evolved with changing environmental situations.

Probability Theory

In the seventeenth century, certain mathematicians, particularly Pascal and Fermat, developed theories concerning the probabilities of the occurrences of certain events in games of chance. These were based upon the symmetry of the finite number of cases possible in, say, a definite number of throws of two six-sided dice, etc. This was enunciated as the probability theory of "equally possible cases" by Laplace in 1814, who wrote:

The theory of chance consists in reducing all the events of the same kind to a certain number of cases equally possible, that is to say, to such as we may be equally undecided about in regard to their existence, and in determining the number of cases favorable to the event whose

³² E. R. Lankester, *The Advancement of Science* (London: 1890), quoted in L. Buermeyer, et al., *An Introduction to Reflective Thinking* (Boston: Houghton Mifflin Company, 1923), p. 173.

probability is sought. The ratio of this number to that of all the cases possible is the measure of this probability.³³

There are clearly useful features in prediction based upon such a theory, but being founded on games of chance where "equal" possibility is readily apparent, it does not point the way to the determination and recognition of such instances in scientific problem solving. However, based on such origins in notably "unscientific" activity, the theory has been developed into statistical methods of considerable power in estimating experimental error, and predicting, within certain limits, the future states of a phenomenological system. In all experimental work, errors are inherent in observation, and in order to increase the value of the solution as far as measures of reproducibility are concerned, estimates of these errors must be made by tests of significance. There always exists a probability of error in a solution. Modern statistical methods, utilizing the ratio of the observed frequency of an event to the total number of instances, generally define probability as either the limit of this ratio as the number of events tends to infinity, or as a *number* associated with that case. Which of these definitions is correct need not concern us here. Certain mathematical axioms are defined for manipulating such numbers, in order to arrive at certain predictive conclusions. Such manipulations definitely do not replace the other methods of problem solving and, being based on certain assumptions of statistical regularity, are not yet completely applicable to problems of organized complexity. "The probabilities have their counterparts in observable frequency ratios, and any probability number assigned to a specified event, must in principle, be liable to experimental verification."³⁴ The use of the methods of statistics leads to the design of experiments to accomplish our problem solutions. The proper design is ordinarily the most economical approach in any given instance, economical in time and in personal and physical resources.

For example, the traditional style of experimenting by trying first one thing, then if that does not work then something else, and so on,

³³ Laplace, *Philosophical Essay on Probabilities*, trans. F. W. Prescott and F. L. Emory, quoted in Boynton, *op. cit.*, pp. 623ff.

³⁴ H. Cramér, *Mathematical Methods of Statistics* (Princeton: Princeton University Press, 1946), p. 151.

in fact the policy of proceeding by small steps, is quite unsuitable. On the contrary, we require to sit down and appraise all the points that appear to be of interest, or likely to be of interest in the future, and then experiment on them all simultaneously. We thus move in jumps rather than steps. The great advantage of moving in jumps is that the information given by a jump adds up to very much more than that given by its component steps, though requiring no more effort.³⁵

The planning and design of experiments in accord with these powerful statistical techniques require knowledge and understanding of multiple correlation, analysis of variance in multi-variable systems, factorial design, and so forth. It is not within the scope of this book to consider these in detail, but we may say that this type of knowledge is of great importance to the modern experimenter and researcher. In general, it may be said that the proper utilization of statistical methods in research can lead to the extraction of the maximum amount of information from given observations and to the solution of problems of unorganized complexity.

Associated closely with statistical methods are operational concepts as developed by Dr. P. W. Bridgman and others. In this approach to the method of science, all concepts are defined in terms of the operations (*actual* methods of attaining a given empirical result) which are used to distinguish or identify them from out of the whole field of concepts. Of course the terms of the operations themselves are subject to operational definition. For this reason, no matter how far we care to pursue our operational analysis, we arrive eventually at a point where we are willing to accept definitions in intuitive instead of in operational terms. There is thus a certain amount of haziness, or perhaps ambiguousness, about experimental and conceptual knowledge which must be accepted. There is no harm in this, but perhaps a great deal of good. A completely rigid operational system would leave no room for advances. However, the operational concept leads directly to the relative character of knowledge, and therefore to the negation of the word "absolute." In turn, operational analysis leads to an examination of meaningful and meaningless questions. For any question to have meaning, it must be

³⁵ K. A. Brownley, *Industrial Experimentation* (Brooklyn: Chemical Publishing Company, Inc., 1947), p. 142.

possible to establish operations by which an answer may be found for it. Thus operationalism is essentially a critical examination of method—all method—and when undertaken in statistical terms can lead to a high order of reproducibility for problems solved in its framework.

Summary

Let us now summarize the methods which we have found to be applicable to the solution of research problems in this brief survey. We may then observe the deficiencies contained in this pattern for the solution of various classes of problems defined in the previous chapter. A regrouping and a comparison of the methods we have already traced with two modern conceptions of the problem-solving process will lead to the desired definition of the "systematic manner," which will be suitable for utilization in efficient research.

The following concepts are either desirable, useful, or necessary in solving the problems of research:

1. *The assumption of causality.* This postulate is necessary, since to deny it would be to deny the possibility of problem solution. The ultimate "truth" of cause and effect, or whether the causality is that of deductively formulated theory of the abstractive type (from natural models) or of deductive postulations from unobservable entities and relations, need not concern us here.

2. *The requirement of basic intuitional postulates in initiating inquiry.* This is tantamount to recognition of a problem, which, though it may be hazy or vague, is nonetheless required if we are to achieve any solutions.

3. *The desirability of observation of the phenomena involved in the problem.* The injunction here is to obtain all the relevant facts. Data obtained from experiment are included. Experiment implies a knowledge of the magnitude of error and the pursuit of relevant knowledge. The determination of relevancy may be intuitional as well as mathematical. In many problems direct observation of data is not feasible, but the collection of postulated relationships regarding the phenomenon is included here. Also implied is the provision of systems of measurement, with recognizable features of resemblance.

4. *The desirability of classifying the observed data.* The observation of similarity and difference, as well as analogies, is one of the first steps to knowledge of any kind. Reasoning by analogy can be quite helpful in the solution of many problems if proper transformations are made under accepted hypotheses in a variety of other fields. Analogy can be dangerous when used to excess, and the measure of its efficiency lies in its ability to construct predictive solutions. It is of no value to say that the animal brain is similar to an electronic computing machine, unless the methods of designing such machines can be used, with physiological and psychological data, to reach conclusions concerning an animal's reactions to stimuli, etc. Imagination and idealization of supposed relationships may follow from classification.

5. *The utility of casting aside all previously formulated solutions.* This is the negation of the evolutionary or genetic method and may have some merit in the solution of problems where previously formulated hypotheses are unsatisfactory or undesirable. As Whitehead says, "almost any idea which jogs you out of your current abstractions may be better than nothing."

6. *The desirability of painstaking experimentation for building successively greater generalizations.* This is nothing more than an extension of the desirability of observation. Painstaking experiment, in itself, has no virtue, since problem solutions gained in this manner are no better than the same solutions gained in bold, imaginative experimental advances. The use of this approach should be conditioned by the nature of the problem and the creative mentalities involved.

7. *The advisability of skepticism, or doubting all concepts which are not rationally clear.* This precept may be useful in solving problems deductively, but more often would conflict with the requirement that a problem merely be recognized. The appeal to the reason alone, in solving problems, may be useful in the early stage of a project in arriving at postulates for a plan of attack, to be referred later to experience. The elimination of all experienced data as being invalid is unlikely to lead to completely productive solutions.

8. *The advisability of simplicity in hypothesis.* This principle, commonly known as Occam's razor, and also formulated by

Newton, is by no means necessary. It is useful, however, in maintaining a certain perspective with regard to the utility of a solution. If two hypotheses are applicable, the simplest is clearly the more desirable.

9. *The utility of the evolutionary or genetic approach.* This involves the logico-historical approach mentioned above—the tracing of resemblances in history. Whenever it is applied with success it affords a clearer solution of the problem, based on the unity and continuity of certain kinds of facts, than is afforded by classification. It utilizes observation, comparison, analysis, synthesis, and analogy, and perhaps should not be considered a separate part of the problem-solving method. However, for the sake of completeness, it has been included here. The attempt to trace information in history must be accepted for what it is—an image of our “present hopes, frustrations and despairs.” It has its place, and some research methodologists insist that it be included in every program. The analysis of literature and past records should be considered in the light of their possible unauthenticity as well as the environment in which the work was originally done. Such an approach can serve to furnish some of the information useful in solving certain problems. Literature searches, in any event, should usually be orientative only.

10. *The utility of deductive solutions to problems.* This comprises inferring particular instances from a general premise, or given particulars. It is a method of verbal demonstration or argumentation, and is commonly illustrated by works such as Euclidean geometry. It nonetheless is useful in arriving at consequences from hypothesis inferred from the observation of data, and thereby arriving at particular solutions. It is in the deductive solutions that the fallacy of affirming the consequent, mentioned previously, enters. As an example, let us say, if A, then B. B is the case. Therefore, A is the case. This would be true if A were the *only possible* antecedent for B. The difficulty of making this affirmation leads to the conclusion that the empirical confirmation of B does not “formally guarantee the truth of the postulate A.” Deductive solutions include both the postulation of concepts by imagination, as exemplified in model theories and concepts which involve intellectual abstractions.

11. *The utility of inductive solutions to problems.* This is, in essence, a method of data classification. It is useful in conjunc-

tion with statistical analyses in arriving at intuitionally significant inferences from observations. Briefly, it includes (as outlined by J. S. Mill):

a. The method of agreement. "If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree is the cause of the given phenomenon." It would be virtually impossible to observe this principle completely, and its usefulness is restricted to those problems where the cause is outstanding. Analyses of off-quality production, in which one factor, such as a raw material, or a particular piece of equipment, etc., is found to enter into each off-quality product, and is thereby accepted as the cause, is typical of this type of reasoning.

b. The method of difference. "If an instance in which the phenomenon under investigation occurs and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ is the effect, or the cause, or an indispensable part of the cause of the phenomenon." The difficulty here is similar to the previous case. The simple controlled experiment where all relevant factors are maintained constant and the addition of one element produces the desired result, and is thereby postulated as the cause, is an example of this method.

c. The joint method. "If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets differ is the effect or the cause, or an indispensable part of the cause of the phenomenon." The method is useful in those cases where several factors are common to a set of instances in which the same result occurs. If instances are found in which they are present and the result does not occur, they may be eliminated. The difficulties involved in distinguishing these factors are clearly apparent.

d. The concomitant method. "Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenom-

enon, or is connected with it through some fact of causation." This is most useful in the hypotheccating of problem solutions based on exploratory experiments.

e. The method of residues. "Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents." The complete use of this method is rare, its partial use an everyday occurrence. It is particularly useful in analyzing data of problems involving deviations from normal or expected behaviour.³⁶

The important factor in the utilization of the inductive solution is the determination of relevant instances. The determination of such relevance is definitely not formal—"common sense, accumulated experience and knowledge, some originality, and a spirit of adventure" are used for the determination of relevancy.

12. *The utility of statistical theories.* Theories of statistics and probability are extremely useful in obtaining the maximum information available, where the assumption of randomness and/or regularity is valid. The concept of theoretical probability has no necessary meaning of itself, and presupposes the assumption of a number of principles of necessary connections. Its use permits the retention of the principle of causality in a number of problems, as in quantum mechanics where the probable aspects of the states of a physical system are utilized in place of time relationships. It also permits manipulation of postulated entities exhibiting indeterminacy (*viz.*, an electron cannot have both a sharp position and a definite velocity at one and the same time) for the solution of certain types of problems. It is also useful for the solution of problems in which the data are statistically regular, and the sample observations sufficiently random—in other words, problems of disorganized complexity. It is an essential part of the design of the most economical experiment in a given problem.

Our analysis of the development of the scientific method has led to the outlining of certain methods of solving research problems. The question of the ultimate truth of the solutions obtained has been touched on but has not been considered relevant

³⁶ J. S. Mill, *System of Logic*, 8th ed. (New York: Harper & Brothers, 1900), Bk. III.

to our purposes. Of all these methods, or ingredients of method, the only ones considered essential are the assumption of causality and the definition of the problem. The others may, and should be, utilized where the conditions of the particular problems warrant. We have attempted to indicate the general types of problems where a given method would be useful or efficient. When the synthesis of a total methodology underlying the entire research process is discussed, it will be seen that the choice of a particular method plays a large role in the attainment of research efficiency. Two modern conceptions of method will now be examined, with a view to determining the completeness of the concepts outlined above.

Modern Method

NORTHROP

Northrop, in his *Logic of the Sciences and Humanities*, emphasizes the analysis of the problem as the basic step in its solution. He divides the complete analysis of inquiry into the following stages:

First, the discovery by analysis of the basic theoretical root of the problem;

Second, the selection of the simplest phenomenon exhibiting the factors involved in the difficulty;

Third, the inductive observation of the relevant factors;

Fourth, the projection of relevant hypotheses suggested by these relevant facts;

Fifth, the deduction of logical consequences from each hypothesis . . . permitting it to be put to experimental test;

Sixth, the clarification of one's initial problem in the light of verified hypothesis; and

Seventh, the generalization of one's solution by means of a pursuit of the logical implications of the new concepts and theory with respect to other subject matter and applications.³⁷

Nothing is found here which is additive to the factors we have listed. Northrop includes all of them in a complete inquiry, capable in his opinion of attaining to a high level of truth value. This only implies for our thesis that such a complete program

³⁷ Northrop, *op. cit.*, p. 28.

is unnecessary for the solution of a problem, but desirable if we are to obtain solutions with the highest measures of reproducibility. We have included the analysis of the problem in the recognition of the problem by intuitional postulates and in the collection of postulated relationships under observation. Northrop also indicates that the proper comprehension of the stage of inquiry in a particular problem is of the utmost importance in obtaining a satisfactory solution. He points out that there is no *one* method of inquiry—all the ways of observing and comprehending relations are applicable, and the efficiency of their use depends on the particular stage of inquiry within which the problem falls. He indicates further that the solution by any deductive methods may be verified by an "epistemic correlation," *i.e.*, the joining by relationship of something intellectually postulated to an intuitive or observable concept. Such a verification would give a solution a definite measure of reproducibility.

CHURCHMAN

Churchman formulates his *Theory of Experimental Inference* in quite different terms. In outlining the methods of "experimentalism," which he finds to be the most valid means for the answering of questions, he examines the logic of making an inference from a series of observations, assuming that "the most precise available methods of drawing inferences from data are those developed by the statisticians."³⁸ He says that the basic postulate of the method of experimentalism is that "There exists a formalization of nature, such that stochastic limits exist for certain sequences of mathematical functions of the observations which are pertinent to a given question of fact."³⁹ He thus implies an assumption of cause and effect, and further that, when an answer to a question (or problem) is obtained by observation from the results of controlled experiment, the solution will have certain statistical implications of reproducibility. A further thesis is that no question is meaningful unless it is purposive. This teleological restriction has been tacitly assumed herein, since a problem was earlier defined as a gap or obstacle barring

³⁸ Churchman, *op. cit.*, pp. 14f.

³⁹ *Ibid.*, p. 173.

the achievement of an objective or goal in the environmental situation. Further,

In terms of the basic methodology of experimental science we can then define the concepts that are fundamental to any theory of knowledge, meaning, truth and reality:

1. No question of fact can be said to have meaning unless its answer is a stochastic limit of an infinite series of observations.

2. No question of law can be said to have meaning unless it forms a part of an image of nature which can be used as a criterion for the adequacy of a set of observations.

3. The true answer to a question of fact is that single value for which the error of observation is zero.

4. The true image of nature is that image which will produce experimental control for all series of observations, finite or infinite.⁴⁰

He continues, from this viewpoint, that both intuitional concepts and rationalistic postulations are necessary in solving problems, to set up formal conditions on a theory of inference which will have a high measure of "truth value" in answering questions. Such concepts fall within the methods we have outlined above, the only question being that raised by Northrop as to the inherent capability of formal probability theory in raising the level of "truth" in the solutions obtained. The answer to this lies beyond the scope of this study, our concern being with sufficient, and not necessarily limiting, answers to problems. As Churchman says,

There is nothing "immutable" therefore in the presuppositions we demand for experimental inquiry; there is no true beginning-point to science. The only invariant to be found is the purpose of all such inquiry; even this is variant with respect to its defining, in the sense that we can become more and more precise as to the meaning of the principle that the end-point of science is the reduction of error to zero.⁴¹

Conclusions

We have here considered, not the mental *act* of problem solving (discussed previously), but some external *methods* imposed on that act, or, "the systematic manner of the application of

⁴⁰ *Ibid.*, p. 183.

⁴¹ *Ibid.*, pp. 209f.

intelligence." We have analyzed various studies of knowledge and logic to obtain a consensus as to methods considered suitable for answering questions at various times. Numerous ingredients have been discussed, and some have been considered requisite, some desirable, and some merely applicable. An understanding of the various methods which may be utilized to obtain data and resolve research problems is essential to the administrator in this field. This summary is intended to provide a brief background to aid in attaining this understanding.

In summary, the systematic manner we would require of the research process comprises

1. The abstracting of information from the environment, based upon certain definite presuppositions, one of which *must* be the assumption of causality.

2. The development of a hypothesis by intuition, based upon analysis and observation (this is a requirement that the problem must be defined, no matter how vaguely).

3. The manipulation of the observed data, or the intellectually postulated relationships, or the intuitive concepts, in some definite manner (a number of variants of manipulatory methods have been described, and the future will undoubtedly see the development of others), consistent with the presuppositions, to arrive at a conclusion.

4. This conclusion, on which action may be taken, is considered the sufficient solution to the problem.

5. The method utilized to abstract information and to manipulate this information must have some implications of reproducibility in order to be suitable to research problems.

Our next objective is the synthesis of the elements of the research process, *i.e.*, intelligence, problems, methods, and solutions, into a rational methodology which will serve as a guide in the establishment of principles to increase the efficiency of specific collective research.

CHAPTER IV

THE METHOD OF RESEARCH

The Research Process

An analysis of the research process indicates that it comprises, in combination, the following elements: creative mentalities, properly defined problems, systematic problem-solving procedures, and certain types of possible solutions. All these elements are necessary and sufficient. Therefore, the most efficient relative utilization of each will make for the most desirable research procedure in any given application. It is the purpose of this chapter to discuss possible principles to be used in arranging these elements in an efficient manner, which will be useful in comprehending the present practice and the future of research.

It should be noted at the start that these principles, intuitively and deductively inferred from an investigation of the characteristics of the elements of research, are to be considered tentative; the implication is not that they are necessarily invariant in research, but that the elements from which they stem are the same within any reference frame. Even the fixed components are subject to modification in range and intensity. For example, the limits of creative ability in problem solving can by no means be definitely defined, and evolution and mutation of the physiological elements involved could change them greatly. The intelligence of an Einstein, say, may or may not be within the ultimate order of magnitude which we should look forward to. Also, even those systematic methods which are known and useful are not fixed or immutable; problems and their satisfactory solutions are determined by changing environments. Therefore, these principles are intended to present a tentative rationale which, if applied and utilized to provide a means of measurement of problem-solving efficiency, will be useful and will, at the same time, serve as a basis for a sharper formulation of the theory of the research process.

Solution of Research Problems

Principles for guiding research will be determined to some extent by our answer to the question: Given a research problem, of what does a solution consist? To guide the process of research efficiently, we must decide what characteristics an answer must have. If, for example, we were to define a solution to a given problem as that resolution of the environmental situation in which the individual or group acts as though it had achieved a "truthful" answer, we have not bounded the process sufficiently to judge the efficiency of the procedure. If a completely incorrect solution serves as the basis for action, it would seem desirable that we should be able to say that the process used had a very low, if not a "zero," measure of efficiency. Conversely, it appears desirable—and reasonable—to say that the more nearly correct the solution obtained is, the more efficient is the research process utilized.

"Correctness" may be determined by the utility of the answer, and not by its ultimate truth value. Thus, many hypotheses, while they were far from being "correct" in the light of present knowledge, were used to resolve problems in vacuum-tube circuits in the early development of the radio and electronic industry. In so far as research is concerned, it is the correctness of the solution in terms of satisfactorily overcoming the immediate gap in the environmental reference frame, which defines the efficiency of the process. The evaluation of efficiency from the broader aspect of science as a whole, or society as a whole, is quite another matter, and one which will not be considered here.

We shall, therefore, consider that a solution to a research problem has been obtained when the individual or group is able to overcome the gap or obstacle in the particular situation. More precisely, the efficient solution comprehends that measure of reproducibility which completely resolves all the elements in the given problem. An incorrect solution would have a measure of reproducibility of zero (this is a probability measure and does not imply complete nonreproducibility), and the correct solution would have total reproducibility *in so far as the visualized elements of the particular problematic situation are concerned. The research process should be so organized as to give solutions*

of the required order of magnitude of reproducibility. Thus, as among research groups, that one will be most efficient, from our point of view, which resolves most of the elements of a particular problem.

Reproducibility here implies that solutions may be more or less complete; it is a scalar factor, and its magnitude depends upon the correctness of its prediction of results which will occur if action is taken based on the solution. For example, a particular research group studying the physics of the solid state and devoting its attention to the solution of the problem of the causes of fracture in metals might determine an answer in terms of impurities in the crystalline structure. This answer is reproducible if, when the impurities indicated are eliminated, fracture of the type under study no longer occurs. If the same type of fracture does occur, but only under higher loadings, then all the elements of the problem have not been resolved; but if these higher loadings do not enter into the problematic situation as it is conceived by the research group—if it is not necessary that fracture under higher loadings be eliminated—then the solution has a reproducibility of the required order of magnitude.

The lack of reproducibility does not necessarily mean that the solution proposed has no value. If the elimination of impurities in the crystalline structure has no effect on the observable variables involved in the fracture, the solution has no measure of reproducibility with respect to this problem. We should realize, however, that it may well be valuable from the standpoint of broad theory, since it might be a step in the long-range development of a particular body of knowledge. Negative or limiting answers may also have implications of reproducibility; in this same case, if the hypothesis were that there is no apparent method of increasing resistance to fracture, action can be taken based upon this solution.

The value of a solution to a research problem may make itself apparent in various ways. The resolution of a particular problematic situation will, in turn, create new problems. The answer which is satisfactory in a given instance may be the focusing of the individual or group attention upon an element or elements of finer detail which were not discernible in the original problem. Such an answer also has implications of reproducibility in that it serves as a basis on which further action is predicted.

As John Dewey says, all scientific operations "are such as disclose relationships . . . which become the means of control of occurrence of experienced things. . ." The fact that research which was thought crucial may turn out to be an intermediate step in the solution of the problem serves to emphasize the relatedness of scientific investigations. In whatever terms the answers to a problem are formulated by the research process, the value or utility of these answers in predicting the results of action undertaken as their consequence is indicative of the efficiency of the process itself. And this is the only indication.

The utility of a particular solution to a problem, obtained through research, in enabling an individual or group to take satisfactory action based upon the future results predicted, is the measure of the over-all efficiency of the process. Measures of this efficiency are no doubt difficult to obtain. Nevertheless it is apparent that two possible types of solutions should be readily recognizable: gross errors and solutions with high degrees of reproducibility. If such a system of evaluating the efficiency of a research organization were practiced, refinements could be developed, based on experience and observation, which would better indicate operational characteristics and the relative difficulty of problems than the qualitative methods for such evaluation now in use. Quantitative means are now in use in some industrial organizations, for example, to measure the results of research, but these are generally based upon some type of profit-and-loss calculations which are at least once removed—perhaps more so—from the research process itself. The measurements we have defined are in some degree related to these latter, but they are in no wise the same. Direct application of these concepts to industrial research will not be considered here.

The other elements of the research process must be considered from the standpoint of the solutions reached. Any principles inherent in the utilization of each of the components must be directed toward the production of reproducible solutions. As previously noted, the research process begins with a problem of a particular type; either the data or the methods of solution are not simultaneously known, or, if known, they are not desirable. Also, systematic methods of solution begin with the problem, its analysis and definition. From the standpoint of ultimate

results, this problem analysis is quite possibly the most important element in the entire process.

Analysis of Research Problems

The first step in the analysis of any problem presented to an individual or group for solution by the research process is a determination as to whether or not the problem is one suitable for the organization. For example, if it is a routine problem for which both the data and method are available and adequate to give the solution with an acceptable risk of error, it would be inefficient for an organization established for research purposes to expend its resources in attaining a solution. A research organization exists for the purpose of using creative abilities to solve research problems, and the decision as to whether the problem is suitable or not is often relatively easy to make. The principle should thus be observed that *a research organization should undertake the solution only of research problems*. The simplicity and obviousness of this concept are deceptive: in industrial organizations, particularly, much work is done in the name of research which involves none of its elements.

Having made certain that a problem requires the resources of the research process for its resolution, the next analytical consideration is the type of relationships required in its solution. Is the problem one in which the correlation of simple variables will be sufficient, or are elements of organized or disorganized complexity involved? The decision reached at this point will be all important in outlining the procedure to be followed in the collection of data and achieving the required relationships. Systems of measurement necessary to obtain data for problems involving simple variables may be, and often are, quite complex, but the methods of manipulation of these data in order to make consequential inferences are straightforward and relatively simple. On the other hand, in problems of disorganized complexity, the (assumed) random nature of the data adds to the difficulty of their collection. Manipulation of the data, depending as it does upon statistical concepts still in the process of evolution, may also be considerably more difficult. Further, problems of organized complexity interpose even greater barriers between the investigators and suitable means of arriving

at conclusions as well as collecting data. Examples of problems of organized complexity are economic research, market analysis, and opinion determination. The data here are not those of random ordered sets, nor are the manipulatory techniques those of simple statistics.

There is, of course, no different art in the adjustment of an organization to the various types of research problems than in setting up the organization to solve problems efficiently in the first place. The principles are the same. The only change is the situation to which they must be applied. Nonetheless, *the application of the research organization to various types of problems requires a suitable diagnosis of the situation in advance.* Clearly, the organization which undertakes the solution of a problem of organized complexity as though it were one of simple variables is proceeding most inefficiently. Personnel, techniques, and other resources required are apt to be quite different and, unless these are provided in advance, efficient operations will not be possible.

In general, this same analysis should be indicative of the various elements of the problem. Most research problems, particularly those of industry, comprehend more than one specialized branch of science. This is true for problems of simple variables as well as for others. As Norbert Wiener so pointedly says, "If the difficulty of a physiological problem is mathematical in essence, ten physiologists ignorant of mathematics will get precisely as far as one physiologist ignorant of mathematics, and no further." Therefore, the various elements of which the problem consists must be distinguished and provision made for their investigation.

From our standpoint in this study, this means that a research problem can and should be defined in terms of various subordered problems which may or may not fall within several specialized branches of science. In the analysis of any problem it is extremely important that the structural elements be distinguished. These elements may be quite diverse, but the thought-directing vectors derived from their recognition are essential to the creative solution. Each of the structural elements may constitute a subproblem, and in so far as the usual research organization is concerned, it is unlikely that any one individual specialist will

have the necessary background, knowledge, and creative ability to solve all of them.

This distinguishing between the structural elements of the problem is the definitive requirement of problem analysis, and it must be undertaken even though the problem itself is somewhat vague or hazy. This is simply to say that the process of solution must start somewhere, and it will be most efficient to begin the process with those resources which will ultimately be involved before the solution is reached, if possible. The implication is not intended here that these elements can always be solved as independent entities, although this is sometimes possible; what is intended, is that the problem shall be analyzed and resolved into some clear pattern of its elemental parts.

The Research Team

It is unlikely that each of the elements can be solved independently; it is more likely that each of the parts must be resolved cooperatively and coordinately, with the specialists operating as a team. The composition of this team should depend upon the a priori analysis of the problem. It will be necessary that the members of the team communicate with each other, each seeing his own work in relation to the whole, and each understanding enough of surrounding fields to suggest experiments to those who have the specialized skill to carry them out. The professionalized (or, in organizational terms, functionalized by skills) approach, which is used by most research organizations, is not the most efficient method of solving problems involving a complexity of elements, and the greatest number of industrial problems are of this type. *There is a particular composite of creative mentalities which will be most efficient in a given instance.*

The application of this principle in a given organization, while dependent upon the analysis, will also be controlled by the range of resources available. Its use will require, in addition, the treatment of the individuals who form the organization as independent entities, to be utilized as the analysis of a given problem dictates, and not necessarily as occupying a particular location in a formal and rigid structure. That this is not a standard or usual *modus operandi* and that it requires a novel conception

of organizational structure and practice is recognized, but its utility and necessity are becoming more and more evident. Its necessity proceeds from the premise which we make here, that *from a definition of a research problem in terms of a set of problems, the scientific resources and environment optimally suited to its solution may be formulated to a greater or lesser degree.*

The ability of an individual to analyze an environmental situation correctly depends in large measure upon a predetermined attitude or "set." Experimental psychologists have termed this mental condition *Einstellung* and have established its existence as a part of the process of thinking. In a sense, the historical position of the research worker with respect to the given problem will have a definite bearing upon his resolution of it into elemental components. The broader the scope of his background and knowledge, and the wider his acquaintance with scientific fields outside his own specialty, the more useful will be the attitude he brings to bear upon the analysis of a problem. It is this "set" which largely distinguishes the laboratory researcher from, say, the development engineer. This is intuitively recognized throughout research, as indicated by such common statements as "The theoretician has an entirely different viewpoint [attitude] as compared with the applied scientist." Each time a problem is reoriented and the structural elements reformulated, the attitude which could most profitably be utilized for further analysis may be somewhat different. Obviously no one individual can be expected to have a multiplicity of such sets toward a given problem. A group is more plastic in this respect, since it is quite possible to shift the collective viewpoint or attitude with the requirements of the situation.

The Collective Attitude

We are confronted, then, with these requirements for problem solving: There is a particular composite of creative mentalities which will be most efficient in a given problem situation; resources and environment for problem solving may be predetermined; it is necessary to analyze a problem into its elemental components before proceeding with the choice of the creative mentalities and the resources for its solution; an individual will bring to a problem a definite attitude or "set" which may or

may not be the most fruitful for the analysis of the situation. We would like to advance the thesis that *the collective attitude of the research team, selected on the basis of the requirements of the problem, offers, in general, the most efficient group of presuppositions from which to proceed to its solution.*

On what basis is the group attitude conceived? At no stage in the research procedure can the individual mentalities be completely blank, and each individual will come to the group with his own particular set. How may these different attitudes be subsumed in a collective set? If the analysis of the problem is undertaken on an authoritarian basis, by a director or leader, and transmitted without modification to the group, then one of the advantages of group activity is lost, and except for the *quantity* of work involved, the research might as well be considered individual. Under exceptional leaders, such a procedure may be and has proved to be quite efficient and can lead to satisfactory solutions. If this transmitted viewpoint is contrary to the collective attitude, the conflict inherent in such a situation could possibly negate the anticipated results of the most brilliant analysis.

In some cases the group approach can indeed simulate the Baconian requirement that all previously formulated solutions be cast aside. Such a situation would arise if opposing attitudes or viewpoints within the group were such as to reduce or negate the over-all set. On the other hand, mutual reinforcement of ideas would tend to strengthen a particular approach. The conception that efficiency would be increased by the use of a collective point of view presupposes that the selection of the group members is sufficiently randomized to cover a broad field and that the personalities projecting individual attitudes are equal in strength. Neither of these assumptions can be completely realized. The spread of education and transmission of information, and the proper selection of research personnel, can do much to assure the existence of the former condition. As for the latter, the influence of personality within a particular group can be utilized advantageously by proper administration and direction on the part of the leader. This is a most important concept in the organization and administration of research activity.

In general, the analysis of a problem presented to a group of research workers may be developed in one of the following ways:

1. The analysis may be made completely by higher authority and a predetermined attitude presented to the group.

2. The individuals may work independently, proceeding on the basis of their own presuppositions.

3. Individual ideas may be presented to higher authority, where another "set" will be added (or subtracted) and the synthesis returned as the attitude to be followed.

4. The group, including such higher authority as may be required, may collectively arrive at a satisfactory analysis of the problem for further work. If proper safeguards are provided in the selection of personnel and the influence of individual personalities, this is the most efficient general principle of operation to be followed at this stage of collective research activity.

The use of this last concept in research presents the same type of organizational and administrative problems discussed in connection with group attitudes above.

This process of analysis and definition is never completed until the problem has been finally disposed of and, at each stage in unfolding and evaluation, reformulation and reestablishment of the collective attitude will be possible and useful. Each new set of empirical observations, each newly formed postulated hypothesis will modify the environment of the problem so as to enable the research group to shift the experimental "spectrum" toward, or possibly away from, the direction of the desired result. *The importance of this analytical and evaluative process as a continuing activity through all the stages of a given instance should not be overlooked in the establishment of research principles.*

The individual creative ability is an essential element in the over-all process we are synthesizing. The selective grouping of such mentalities and the utilization of their individual attitudes to create a more effective group approach have already been discussed with respect to a rational analysis of problems. What of the individuals themselves? What principles should be kept in mind in order that their abilities be utilized most effectively?

The Individual Researcher

Probably the most important single element in problem solving is the realization by the researcher that any given problem

may differ, from stage to stage in its solution, in method required, and in difficulty of applying that method. It is therefore of paramount importance to the administrator of research personnel that he plan the work of his team with this in mind. Two precepts should be followed in assigning individuals to work on projects:

In the first place, *it is essential that the abilities of the individuals be such that they are capable of assuming the responsibility of solving the problems delegated to them.* Clearly, if an individual, as part of a team or alone, is given an obstacle which he is not capable of overcoming, he is not being utilized efficiently. If he does reach a solution, it will be in a haphazard manner; like the hen who finds her way out of a three-sided enclosure to reach food on the other side, the probability is higher that she would starve before achieving a solution to her problem. Similarly, the creative mentality will be used inefficiently if its capabilities are far greater than those required to solve the problems presented to it. The latter case is not so serious as the former for a particular research organization; it is wasteful of resources, but at least the desired objective will be reached.

In the second place, *in weighing the ability of an individual it is important to realize that creative mentalities differ, and the types of mentalities required for different problems, or for different stages of the same problem, may differ.* Thus it may be that one individual could make the necessary intuitive postulates on which to design experiments and collect empirical data, but not be capable of deducing the necessary relationships or making the epistemic correlations required for verification. This is again indicative of the desirability of the team approach, since the flexible research team can be designed so as to include the requisite abilities. In a rigid organizational framework, however, it may not always be possible to select those individuals best suited to a given stage of the problem.

C. E. K. Mees has discussed this distinction between abilities. Speaking of the difference between theoretical synthesis, observation, and experiment, and invention, he says:

Psychologically, each involves distinct methods of working and different types of mind. There is even opposition among them; that is, it is unlikely that one man will excel in more than one direction. It is rare,

for instance, for a capable inventor to be a theoretical thinker. Some scientists excel in their ability to visualize general syntheses and thus evolve theories. Some excel in their skill in observation or in their ingenuity in designing experiments. Some have a capacity for inventing and can design entirely new ways of accomplishing their ends. . . . Scientists and technologists can advantageously be classified according to the extent to which they possess the three scientific factors and the ability to organize.¹

He continues in this vein, classifying Descartes, for example, as a theoretician, Galileo as showing ability along all three lines, Newton as experimenter and theoretician, and Edison as "the inventor par excellence." What makes for these differences among creative mentalities, and whether these are the only ones of importance, are questions open to some dispute, but the recognition of the concept remains.

J. B. Conant makes the distinction between the "scientists who excelled in strategy and those who were outstanding as tacticians." He compares Lavoisier and Priestley as follows:

Lavoisier's lasting contribution was made because he placed his experiments in the framework of an ambitious attempt to explain a great many facts in terms of a grand conceptual scheme. It would not be too misleading to call him a master strategist in science. Priestley on the other hand, probably excelled Lavoisier as an experimenter but he failed to appreciate fully the significance of his results. . . . It is not unfair to say that he was a great tactician, but a poor strategist.²

This is not the place for a fuller discussion of such differences, interesting though they may be. The point is that since such differences in potentiality are recognized as existing among the greatest of scientists, they should certainly be taken into account in attempting to solve problems in a systematic manner with average individuals. *It is necessary to iterate that the greatest single factor in increasing the efficiency of utilization of creative ability in an organization is the selection of problems, and stages of problems, compatible with the capabilities of the individuals in the organization.*

¹ C. E. K. Mees, *The Path of Science* (New York: John Wiley & Sons, Inc., 1946), pp. 60f.

² J. B. Conant, *On Understanding Science* (New Haven: Yale University Press, 1947), p. 100.

The Choice of Method

The minimum essentials in the solution of a given problem are the assumption of causality and a definition of the problem but, once we have these, the choice of specific methodological techniques is quite wide. *The selection of the method will depend upon the analysis of the problem, the individual mentalities involved, and the general character of the team or organization.* The efficiency of the process will vary with the suitability of the method. The use of deductive solutions in a problem of market research, or the use of a strictly intuitive approach to a problem in magnetic field theory could be equally futile. On the other hand, the use of an intuitional approach to a problem by a single individual in an organization, in which all other results are obtained by strict deductive logic based on data at hand, might be the single factor making the solutions obtained useful. Methodology is, and must be, flexible; and while it is possible to state approximately the over-all objectives for the solution of any problem—to analyze the situation, to collect empirical data to obtain relevant information, to manipulate these data in such a way as to arrive at inferences and conclusions, to obtain the necessary epistemic correlations and evaluate results—the pattern from problem to problem may vary considerably.

The choice of a method should be considered in the same light as the choice of any other resource. Method is not the be-all and end-all in research, nor is it a random factor whose choice would in no way affect the process efficiency. The selection of a method in advance of any analysis and the imposition of this method upon what may be considered fixed entities in the system would be highly inefficient. Similar considerations as those discussed in arriving at a collective attitude toward problems hold in the choice of the manner of their solution. It may be that more than one method appears feasible, and if so it will be efficient to try all that do. Changes in the attitudes assumed toward a problem, whether of a part or of the whole, will occur from time to time as the situation is modified by the action of the researchers. Changes in method believed to be the most efficient in a given instance (stemming from reanalyses of the problem) will perhaps occur less often, but not so infrequently that any method should be considered inflexible from start to finish.

Such changes may deal with the methods of collecting valid data or with the methods used to manipulate them. The resources of personnel and equipment involved in either situation may be quite complex. For example, a shift from strictly classificatory to statistical methods in drawing inferences, or a shift from the collection of data involving one or two variables in a laboratory to the collection of data in a plant-scale operation may entail a complete reorientation of an entire research organization. For this reason, an organization is more efficient if it can anticipate the requirements of possible changes in the methods used to solve its problems.

The modes of measurement available to the experimenter or researcher play a leading role in the type of problems he can solve, and his efficiency in solving them. It was not possible for Pascal, for example, to collect the necessary data leading to the solution of the theory of the pump until Torricelli devised the barometer. The selection or availability of devices for the measurement of the data necessary in problem solving is an important factor in the choice of a method. In any measuring device, whether it is one to measure physical attributes or value judgments, there are certain inherent characteristics of sensitivity, precision, and range. In addition to these, the researcher must be assured that the units he is measuring are those relevant to the variables in the particular problem. *Thus, in the ideal case, the decision as to the variables necessary to solve the particular problem, the range of limits of the variations it is necessary to measure, the precision or accuracy of measurement desirable (in other words, an advance estimate of the error), and the sensitivity or response characteristics to a given stimulus would all be predetermined, and the necessary resources embodying the desired characteristics provided.* Such a situation is by no means usual, and often compromises must be made with one or several of these requirements. A more unhappy case, and one which is quite common in research on problems of organized complexity, is that in which the units chosen to be measured are not even relevant to the problem itself.

To a great degree, the experimenter or research group has established the limits of possible results before the solution to the problem has been attempted. It is not always possible to vary the means or instruments available in a given instance.

Nevertheless the type and nature of the problems which can be solved may be outlined in advance by the same type of analysis we have advocated previously, this time considering the problem in the light of the instrumental means available to solve. Exploratory work can be of great assistance and is definitely required in making all decisions of this type. Such work has been tacitly assumed in our discussions as a part of the complete analysis of the problem.

The concept of control is part of the same pattern. Control is not evoked for its own sake—systematic experimentation is purposive. By its use researchers can more efficiently hypothesize relationships and abstract relevant information from observed data. The use of more complex measures of control than are necessary is no less inefficient than the lack of sufficient control preventing the satisfactory prediction of results. Thus, the most refined statistical tests of significance would not have assisted Galileo in reaching more comprehensive conclusions from his experiments upon the motion of an object on an inclined plane. It is better here also to err on the side of excess if a research organization is to attain its objectives. Exactitude is not possible, and *it should be a guiding principle of research to provide sufficient means to perform the measurements necessary to obtain an answer in a specific instance, including the measurement of relevant variables in consistent units with satisfactory range, sensitivity, and control to provide the desired aspects of reproducibility.* Laboratory conditions do not necessarily provide these factors; or, as Churchman says:

. . . we have gradually been led to abandon the older concept of a laboratory science, and to redefine the meaning of experimental control in a more general manner. We have been led to this revision on two counts: (1) it is no longer obvious that laboratory techniques are *sufficient* for answering critical questions, even in the physical sciences, and (2) it is no longer obvious that the laboratory is necessary for obtaining a set of reliable data on a given question.³

By this he means that the conditions within the laboratory may be sufficiently at variance with those of the general environmental situation that predictions based on laboratory re-

³ C. W. Churchman, *Theory of Experimental Inference* (New York: The Macmillan Company, 1948), p. 226.

sults will be unsatisfactory. Further, the development of statistical techniques for arriving at inferences as to the effects of a multiplicity of variables makes it possible to answer questions in situations beyond the confines of rigid experimental design. He continues,

This means that the experimenter must become precise enough as to the meaning of his question so that a set of specific observations can be judged regarding their adequacy, and become general enough in his presuppositions about the natural order so that he can decide whether a continued sequence of observations would approach some limiting value . . . it would be foolish to expect that this demand can actually be satisfied in the early stages of scientific developments. But the process of discovery becomes scientific if and only if such control exists, regardless of whether the investigation takes place inside a laboratory or not.⁴

We have been concerned with the formal aspects of inquiry only in so far as they relate to the actual process of problem solving. Therefore, we may say that a great many research problems may be resolved which are not "scientific" in the above sense. However, there may be problems in which the predictions resulting from the research solution will be in no sense valid without statistical control. Such problems relate particularly to the fields of the social sciences, and to large-scale conceptual schemes in the physical fields, such as quantum mechanics. Where the laboratory results are not sufficiently reproducible to answer a question, resort is made to "pilot plants," "life tests," "sampling techniques," etc. If these give the desired results, well and good. If not, then it is certainly inefficient to employ them. The choice of method should be made on the basis of the analysis of the obstacle in the environmental situation which is to be overcome, and the total resources available to the research organization to achieve this purpose.

Many seemingly factual problems are, when examined more closely, questions of value; value judgments are necessary. As John Dewey has pointed out, a physical standard is not a value, but a physical definition with respect to quantity. Nonetheless, in making many laboratory measurements judgment is required

⁴ *Ibid.*, p. 228.

—a judgment of the value of the coincidence or comparison observed. A child can be taught to take a measurement with a yardstick, but he cannot be taught to evaluate the significance of his measurement. Judgments in general have two functions: either they distinguish parts in the whole or they distinguish a whole among the parts. These can be called the functions of discrimination and unification, or the functions of analysis and synthesis.

The difficulty of establishing valid procedures for the application of value judgments and for the answering of questions of value does not imply that such tests can by any means be eliminated from research procedures. The coordinated combination of judgments of a group selected as best fitted to solve a given problem is probably the best answer that can be given to this question under the present circumstances. The unification of our sciences is one approach. Wiener's *Cybernetics*, combining physiology, mathematical statistics, and electronic theory to form a method capable of dealing with problems of "control and communication in the animal and the machine," is one example of attempted unification. There are others.

The recognition that judgments enter into the research process and that they are increasingly valid as they are made from an increasingly comprehensive point of view, whether analysis or synthesis is involved, is of considerable—if not paramount—importance to research. Any neglect of this will have a definitely adverse effect upon the efficiency of an organization. Further, as the tendency has been to move scientific problems out of the relatively pure air of the laboratory into the "contaminating" environment of the world, and as this "contamination" has made the study of surrounding fields mandatory if valid conclusions are to be reached, it may be (as Churchman thinks possible) that the unification of science is necessary for the progress of science.

Conclusions

In conclusion, the answer to the question of the most efficient manner of arriving at satisfactory solutions to research problems in a collective organization may be given by the following general principles:

1. An active analysis of the problem or problems in terms of complexity, attributes, compatibility with the capabilities of the organization, and structural elements or subproblems.

2. Determination of the probable mental, logical, and physical resources necessary for the achievement of a solution.

3. Continuing reanalysis as results are obtained, and reevaluation of resources required.

4. A unified or cooperative approach utilizing those resources capable of providing a solution.

Further orientation toward the process of inquiry is provided by John Dewey, who describes its general characteristics as follows:

The first is the obvious one that all experimentation involves *overt* doing, the making of definite changes in the environment or in our relation to it. The second is that experiment is not a random activity but is directed by ideas which have to meet the conditions set by the need of the problem inducing the active inquiry. The third and concluding feature, in which the other two receive their full measure of meaning, is that the outcome of the directed activity is the construction of a new empirical situation in which objects are differently related to one another, and such that the consequences of direct operations form the objects that have the property of being known.⁵

These principles will provide a means for concerted endeavor and attack upon all types of research problems. An efficient organization is certainly not one in which each individual is doing the same thing. Each individual must be given responsibility for a certain share in the over-all task, his duties must be defined, and (as we have noted) his capabilities must be suited to those duties. In addition, he must understand what is required of him. The task of delegating and defining these responsibilities so as to carry out the collective research process in accordance with these principles falls within the province of administration and organization. A knowledge of the background of creative ability and the scientific method is essential as a guide to administering those who advance scientific knowledge by means of research.

⁵ John Dewey, *The Quest for Certainty. Intelligence in the Modern World: John Dewey's Philosophy*, Joseph Ratner, ed. (New York: Modern Library, Inc., 1939), p. 318.

CHAPTER V

THE BACKGROUND OF RESEARCH IN INDUSTRY

Evolution of Industrial Research

We have seen that man has been solving problems of one type or another as far back in his history as we can trace his efforts. In a broad sense many of these have been the problems of industry, but his procedures have not, until recently, been those of the research process as we would define it. We have indicated also that the development of modern industry (the utilization of capital, labor, and machines for the production of salable goods) was approximately coincident with the growth of research. The interaction of these two progressive factors is not to be denied. In any case, the beginnings of both were unsure and uncertain, and the exact point of merger of the awakening interest in the phenomena of nature with that in the expansion of commerce and trade is certainly indeterminate. Nonetheless, these all-important influences in the evolution of our present civilization interacted slowly, but with accelerating momentum, to evolve industrial research as we know it today.

In accordance with our earlier analysis, *industrial research may be defined as the application in industry of human intelligence in a systematic manner to a problem or problems whose solution is not immediately available.* An interesting early example of the systematic approach to problems of industry is given by Agricola's works on mining and metallurgy in the sixteenth century. He attempted to investigate, methodically and experimentally, all the information then available on the subject. Mining had been practiced for centuries, and a body of practical and useful art had arisen in connection with it. Agricola, who had been trained in the best scientific tradition of his times, turned his creative abilities to the problems which had accumulated through the years of experience. His *De Re Me-*

tallica was a comprehensive scientific treatise of the highest order, and many of the general precepts in the Hoover translation of this work read as though they came from a present-day text. For example, Agricola says:

Many persons hold the opinion that the metal industries are fortuitous and that the occupation is one of sordid toil, and altogether a kind of business requiring not so much skill as labor. But . . . it appears to be far otherwise. For a miner must have the greatest skill in his work, that he may know first of all what mountain or hill, what valley or plain, can be prospected most profitably, or what he should leave alone; moreover, he must understand the veins, stringers, and seams in the rocks. He must be thoroughly familiar with the many and varied species of . . . rocks, metals, and compounds. He must also have a complete knowledge of the method of making all underground works . . . [and] the various systems of assaying substances and of preparing them for smelting. . . .

Furthermore, there are many arts and sciences of which a miner should not be ignorant . . . the origin, cause, and nature of subterranean things . . . medicine, that he may look after his diggers . . . astronomy that he may . . . judge the direction of his veins . . . the science of surveying . . . arithmetical science . . . that he may calculate the costs to be incurred in the machinery and the working of the mine . . . architecture . . . drawing, that he may draw plans of his machinery . . . [and] lastly the law, especially that dealing with metals. . . .

But as for us, though we may not have perfected the whole art of discovery and preparation of metals, at least we can be of great assistance to persons studious in its acquisition.¹

A more practical and scientific approach can hardly be imagined, and obviously the mining industry of today can ask no more of its engineers and metallurgists. This complete comprehension of the interrelationship (*even to the cost factor*) of science and industry was certainly atypical in Agricola's day but was a forerunner of the unification that was to come centuries later.

¹ Georgius Agricola, *De Re Metallica*, trans. Herbert and Lou Hoover, quoted in H. Boynton, *The Beginnings of Modern Science* (New York: Walter J. Black, Inc., 1948), pp. 583ff.

Economic Motivations

Another example of the commercial motivations of research activity and of the influence of science upon commercial ventures is to be found, as we have noted previously, in the great explorations of the fifteenth century:

[The explorations] . . . illustrate a point . . . in the relations of science to economics and industry: how hard it is to say which is cause and which effect. The *theory* of a spherical earth made a mercantile project possible; this again paid for further scientific research which was necessary.²

The economic motives in the environment grew stronger and, as we noted in an earlier chapter, led to the establishment of observatories for the solution of specific research problems. The movements of certain of the heavenly bodies such as the sun, moon, and planets are readily observable and measurements can be made without the need for experimentation. Thus, it is fitting that progress should have been made in this field even before the comprehension of the need for the experimental approach. The tradition of careful measurement of data concerning these movements extends from earliest antiquity, from the publication of the astronomical tables of the Arabs in Spain in the eleventh, twelfth, and thirteenth centuries through the works of Copernicus, Tycho Brahe, Kepler, Galileo, Newton, and those who followed. Science was being forwarded in a series of solutions of increasing fruitfulness. The influence of this work on the extension of similar methods to trade and industry went far beyond its immediate results in astronomical science. In its wake, the scholarly societies and scientific curricula, which were being established in the universities and among the men of science, began to produce scientists. Many of these were not of genius rank, but of intermediate intellectual status, but their creative abilities contributed in large measure to the beginnings of research in industry.

² H. T. Pledge, *Science since 1500* (New York: Philosophical Library, Inc., 1947), p. 34.

Need for Scientifically Trained Personnel

As Bartlett notes, "Without a fund of scientific knowledge from which to draw and without a supply of men sufficiently prepared to apply that knowledge, the industrial research laboratory could not exist."³ This is pointedly demonstrated by the comparative development of the dye industry in England and Germany in the nineteenth century.

In Germany from the laboratory of Justin von Liebig at Gies-sen, from 1825 onward, scientifically trained personnel went out in increasing numbers to found other laboratories, teach, and also to enter industry. Despite the fact that England had been the leader in industrialization and that by 1850 she was the leading representative of the new techniques, the movement for the establishment of truly scientific institutions of learning had not taken hold. Bartlett quotes Matthew Arnold as writing, "In nothing do England and the Continent at the present more strikingly differ than in the prominence which is now given to the idea of science there, and the neglect in which the idea still lies here. . . ." ⁴ The influence of the environmental backgrounds is clearly evident. Despite the important commercial discoveries and the early establishment of the learned societies in England, the division between the "practical" man and the "scientist" was already becoming definitely marked. This division has persisted to the present day in both England and the United States. A possible cause was to be found in a fundamental misunderstanding by both protagonists as to the general nature of problem solution. As Gotch remarks, "The assault [upon the scientific method] has continued to the present day . . . [and its] characteristics are displayed in the attacks made upon the early work of the Royal Society. . . ." ⁵

Such a state of affairs did not exist upon the continent, and particularly in Germany, where the value of the scientist to ma-

³ H. Bartlett, "The Development of Industrial Research in the United States," *Research, A National Resource*, Vol. II (Washington: U.S. Government Printing Office, 1941), p. 19.

⁴ Matthew Arnold, *Higher Schools and Universities in Germany* (London: Macmillan & Co., Ltd., 1874), quoted by Bartlett, *op. cit.*, p. 21.

⁵ F. Gotch, "On Some Aspects of the Scientific Method," quoted by T. B. Strong, ed. (New York: Oxford University Press, 1906), p. 29.

terial prosperity was early recognized by the various states. For example, the work of Agricola was sponsored by Prince Maurice of Saxony. The School of Mines at Freiberg is said to be the oldest technical "High College" in the world.

DEVELOPMENT OF SYNTHETIC DYES

Thus, despite the fact that the genesis of synthetic dye production lay in the discovery of aniline purple, or mauve, in 1856 by W. H. Perkin, an Englishman, it was in Germany that the most fertile soil for its development was found. A feud had developed early in the eighteenth century between English and German scientists as to whether Leibniz had stolen from Newton's ideas, or independently invented the calculus. This "largely cut English mathematicians off from contact with continental mathematicians for almost a century, during which the latter forged far ahead—to the grave disadvantage of English science." It is extremely difficult to isolate a given circumstance in a total environment and point to it as the underlying influence making for a certain direction of development. However, such factors were undoubtedly influential in particular instances such as this.

Although the manufacture of dyestuffs was mainly confined to England and France for a decade or two after Perkin's discovery, the resources of trained personnel in Germany soon proved to be the overwhelming factor of difference. With the synthesis of alizarine (madder) in Germany in 1868 and the establishment of several manufacturing enterprises there, the early English advantage was rapidly eclipsed. By 1878 Germany had captured over 60 per cent of the world market, and this percentage had risen by 1913 to nearly 81 per cent. Systematic research for development purposes, utilizing laboratory-trained chemists, was apparently well established in the German dye industry. For example, it is estimated that over 4 million dollars was expended on the solution of process problems for the development of indigo production, between the time of its synthesis in 1880 by Von Bayer and the completion of the work in 1897. The subsequent English developments were not even basic, and the dominance of the German organic chemical industry was even greater than indicated by these figures, since most of the intermediates that

entered into the former's production were produced in Germany. The *Encyclopaedia Britannica* points out in its article on "Synthetic Dyes" that

In Germany the organic chemical industry, of which dyestuffs manufacture was the mainstay, was developed on the broadest lines, supported by the banks, and directed by University trained scientists. By 1880 two German works were employing sixty scientific chemists. In 1900 six firms employed 500 chemists, 350 engineers and technologists and 18,000 workpeople. In Great Britain at the latter date the corresponding figures were 30 to 40 chemists and 1,000 workpeople. Whereas between 1886 and 1900 German firms obtained 948 patents for the manufacture of dyes, British firms took out only 86, the ratio of the number of patents closely corresponding to that of the number of chemists employed.

The effects of von Liebig's unpretentious laboratory were far-reaching indeed!

The German development represented a tremendous growth and far outshadowed any other industry, or for that matter, the total research growth for the entire industrial environment of other countries during the same period. This brief résumé of the dye industry's history is far from complete but indicates the basic interconnection between industrial research and the trained scientists, as well as the vitality of an industry under such circumstances. The growth of such research was compelled to be sporadic so long as no systematic approach to problems was available. In general, without training facilities and training incentives for study of the scientific method, personnel to apply the systematic approach was lacking.

STEAM POWER

The tremendous supremacy that England enjoyed in the field of mechanical power with the improvements of Newcomen's early steam engine by James Watt was a haphazard affair, although Watt himself approached the problem somewhat systematically. The types of solutions to the problems of the steam engine were very similar to those arrived at in other instances by the earlier civilizations which we have discussed. The implications of reproducible principles involved in these solutions were neither

understood nor utilized. Notwithstanding Watt's knowledge of the advantage to be gained by using steam expansively, he made no attempts to consider the use of higher pressures. The relationship of heat in the expansive fluid to the work involved apparently was not at all visualized by either the inventors, the manufacturers, or the users of steam engines during the eighteenth century.

The basic problem, being at best vaguely realized, could not be attacked in any systematic manner. For this reason, perhaps, there was no evolution of a supply of scientifically trained personnel to engage in research in mechanical power. Watt's first engine was invented in 1763, and it was not until 1824 that Carnot published his *Reflexions sur la puissance motrice du feu* which developed the theory of the steam engine as a heat engine. Even so, Carnot apparently was unaware of any losses in the process, and it remained for Mayer and Joule in the 1840's to visualize and measure the mechanical equivalent of heat.

From the middle of the nineteenth century onward, the science of thermodynamics developed rapidly, and it may be said that industrial research was under way in this area. Joule, a physicist and brewery owner, was particularly interested in the construction of electromagnetic machines, being motivated, as he wrote, by the industrial investigations which were being carried on at the time. "I am particularly anxious to communicate any new arrangement in order, if possible, to forestall the monopolizing designs of those who seem to regard this most interesting subject merely in the light of pecuniary speculation."⁶

Mayer, on the other hand, was a physician, and during a stay in Java was led to remark on the particularly bright red color of the venous blood of some of his patients. He hypothesized that this brightness was due to the fact that a less amount of oxidation sufficed to keep up the temperature of the blood in a hot climate than in a cold one. From such unlikely beginnings began his interest in, and experiments on, the mechanical equivalent of heat. By 1842, as Tyndall remarks, "this obscure Heilbronn physician . . . was in advance of all the scientific men of his time . . . as regards the mechanical theory of heat."

⁶ John Tyndall, "Copley Medalist of 1870," *Fragments of Science*, 5th ed. (New York: Appleton-Century-Crofts, Inc., 1877), p. 268.

Although perhaps a digression at this point, it is most interesting to note Tyndall's comparison of the two men in the Copley Medalist Address of 1871:

The differentiating influence of "environment" on two minds of similar natural cast and endowment, comes out in an instructive manner. Withdrawn from mechanical appliances, Mayer fell back upon reflection, selecting with marvellous sagacity, from existing physical data, the single result on which could be founded a calculation of the mechanical equivalent of heat. In the midst of mechanical appliances, Joule resorted to experiment, and laid the broad and firm foundation which has secured for the mechanical theory the acceptance it now enjoys. A great portion of Joule's time was occupied in actual manipulation; freed from this, Mayer had time to follow the theory into its most abstruse and impressive applications. With their places reversed, however, Joule might have become Mayer, and Mayer might have become Joule.⁷

The general influence of environment upon the work of a researcher has been noted previously. Whether we agree with Tyndall as to the consequences in this instance, the work described in conjunction with that of other contemporaries led to the appreciation of the First Law of Thermodynamics, and the formulation of the Second Law. Although these theories were not well received at the time they were expressed, being considerably in advance of those enjoying general vogue, they, nevertheless, gave impetus to experimental work on heat engines. After the middle of the century, students and followers of Mayer, Joule, Clausius, Rankine, and others were available to enter industry and establish power research upon a systematic basis. Rankine's *Manual of the Steam Engine*, the first systematic treatment of steam-engine theory, was published in 1859. The history of this particular development will not be followed further, but it will suffice to note that the lag in the supply of trained personnel, and the "practical" tradition which had grown up, limited the growth of research in this field until quite recent times. In fact, it has never quite attained the status that research enjoys in chemical industry.

⁷ *Ibid.*, p. 283. The calculation referred to concerns the difference between specific heats at constant volume and constant pressure. Mayer showed that this difference was due to heat absorbed by the gas in expanding against pressure.

The internal-combustion engine followed a similar course of development. The first such engines were gas engines similar to steam units, and by 1820 a working model had been described in England. They were considerably improved thereafter, but it was not until 1862 that a general theory was developed by de Rochas in France. Even then, this was not utilized industrially until 1876, when Otto produced an engine embodying the Rochas cycle. Further developments were probably hampered by lack of proper personnel. For example, it was only in 1892 that Diesel patented the compression ignition engine.

ELECTRIC POWER

Electromagnetic developments on the other hand followed quite a different course. Based on Faraday's strictly scientific investigations, as well as those of Henry in this country, a body of theory had been built up before an industrially useful machine was invented. In a story, possibly apocryphal, Professor Kendall describes a discussion between Faraday and Gladstone: "Faraday discovered the principle of the dynamo and electric motor in the 1830's. Gladstone, as Chancellor of the Exchequer, asked, critical of Faraday's experiments, 'What is the use of electricity?' Faraday replied, 'Someday, Mr. Chancellor, you will be able to tax it.'"⁸

Within forty years, the methods propounded from these and other theories, as well as the personnel trained in the science, had so increased and developed that electrical generator efficiency had been raised from less than 50 per cent to over 90 per cent. Hopkinson, in 1886, gave a complete method of calculating generator performance and placed industrial design on a rational basis. In such an atmosphere, it is not surprising that active research and development should have been begun quite early by those industrial enterprises engaged in this work and in the allied field of communications. Germany had established, following the Franco-Prussian War, *The Reichsanstalt*, an institution devoted to research and investigation in the physical sciences and their applications. Trainees and workers from this

⁸ B. Lovell, *Science and Civilization* (New York: Thomas Nelson & Sons, 1939), p. 105, quoting Professor J. Kendall, "Royal Institution Christmas Lecture," 1938.

institute were used to establish the research departments of such electrical firms as Allgemeine-Elektrizitäts-Gesellschaft and Siemens and Halske. In the United States, the work of Weston, Edison⁹ (who set up his laboratory at Menlo Park in 1876), the Bell Telephone Laboratories, the General Electric Company,¹⁰ Westinghouse, etc., in establishing research laboratories is indicative of the trend in this industry, and of the essential requirement of creative mentalities plus systematic methods in all research.

TEXTILE INDUSTRY

One more industry will be considered in this development of the industrial research background, and it is fitting that we note the advice of a financier in that industry (textiles) to Elihu Thompson of the General Electric Company concerning the establishment of further research facilities. This textile industrialist "told Thompson [in the 1890's] that he thought the electrical industry was rapidly becoming standardized and getting to the point where new research and experimentation were hardly necessary. . . ." In so far as the textile industry itself was concerned, this had been the attitude for generations and, as a consequence, no serious attempts had been made to develop a body of science, or scientifically trained personnel, for research in what might have been an extremely fruitful field. The Arkwright spinning process and the Cartwright power loom, both of which were in use prior to 1800, were remarkable developments for the time. However, they have been basically so dominant in the field and, in a sense, so satisfactory, that until quite recently there appears to have been no industrial incentive to provide any of the essentials of the research process.

⁹ Edison has "often been criticized for his 'trial and error' methods. [However] . . . back of everything he did or tried there was always an idea. The starting point was always the need . . . the second stage was the suggestion of various ways of accomplishing that purpose, and the final stage consisted in trying these suggested solutions in as thorough and systematic a manner as possible. . . . Such a procedure can be found in any industrial research laboratory today." National Resources Planning Board, *op. cit.*, p. 31.

¹⁰ Dr. Steinmetz was a consulting engineer for General Electric from the foundation of the company, and the research attitude was clearly evident in that company's earliest work.

There have been improvements in both spinning and weaving, of course, but none to compare with those in other fields. As recently as 1940, research personnel in the natural-fiber textile field was numbered in the hundreds, and this in an industry providing a basic requirement for modern man. Except in the development of artificial fibers, a field which is definitely chemical, the availability of all types of scientific knowledge and the course of research in other fields has had little effect on methods of manufacturing yarns, cloth, and clothing. In the present day, attempts are being made to "catch up" by the textile industry. Thus, the following technical associations and institutes have been established, among others: The Textile Foundation, American Association of Textile Chemists and Colorists, the Textile Institute. It is estimated that the textile industry spent nearly 16 million dollars on research in 1946, compared with less than 8 million in 1940. This approximates less than 10 cents for research for every \$100 added to the value of goods in the process of manufacture, compared with some 64 cents per \$100 value added by manufacturing industries as a whole. Other things being equal, it would appear that the deficiency will exist for some time to come.¹¹

Importance of Economic Factors in the Development of Industrial Research

This example is illustrative of the fact that in industry, as elsewhere, a need must be felt before the necessary resources will be provided to carry out research work. It is interesting to note that, of some 50 industrial research organizations described or noted by Bartlett in his article, "Research in the National Economy," not one is in the field of textiles. That this need has not been felt indicates the importance of *economic factors* in industrial research. The situation was not similar to that in the chemical industry where the manufacturing enter-

¹¹ H. Koshetz, "Textile Industry Lags in Research," *New York Times*, Nov. 21, 1948. This article is indicative of the difficulty encountered in establishing research in textiles. For example, "Another factor which makes selling the research program a tough job is the fact that, while manufacturers appreciate research in general, they cannot see its value to their own companies. . . . Additional bewilderment has been created by trite arguments on differences between basic and practical research . . . etc."

prises “. . . gave the chemist finances and a motive.” Textile machinery was relatively inexpensive and highly productive. The resulting products were more than adequate. In periods of prosperity, returns on investment were very high; in periods of depression there was no recognition that improved products or machinery might repay the investment in research. These factors combined to limit incentives to research or even development beyond minor improvements in efficiency and production rates. Apparently, this was what Thompson’s adviser had in mind when he pointed to standardization—the existence of tremendous quantities of more-or-less standard tools of production in an industry serves as an effective bar to the development of scientific methods where none exist. Thus, we may say that *the total environment must be in an acquiescent state before a research attitude can be evolved in a particular industry.* This is true despite the fact that a positive correlation is known to exist between the number of scientifically trained personnel working in an industry and the various efficiency factors in that industry.

Evolution of a General Pattern

The pattern we have described for these representative industries is quite general and applies to the development of research for other industries in the United States as well. Little research was done prior to the previously mentioned establishment of the schools of science and technology in the early part of the nineteenth century. Economic factors, of which the vast amounts of natural resources available and the tariff protecting inefficient manufacturers are the two major ones, presented additional bars to research development. Then too, the English heritage of antagonism to men of science in industry was also part of the national scene. Isolated instances of research laboratories being set up can be found prior to 1860, and these were mainly in the chemical and metallurgical field where the German scientific tradition had considerable influence.

The Civil War had its effect, both in the North and the South, in making manufacturers generally conscious of the possibilities of utilizing the systematic approach of the scientist in solving production problems and developing new products. The basic

industries were occupied in expanding with the needs of the growing country, and the former type of problems was much more important than the latter. The influence of the desire to lower costs and increase production upon such research as was being done is strikingly illustrated by a comparison of research in the metallurgical industry in Great Britain, Germany, and the United States between 1900 and 1928. It was in this industry that an early American research laboratory was established in Philadelphia in 1866. Sisco outlines a measure of research activity for the three countries mentioned in terms of a research factor which is based upon the amount of research work reported and the number of patents obtained in terms of total iron and steel production of the particular country. While this factor is quite subjective and can by no means be taken as an absolute measure, it is, nevertheless, indicative of the type of work which was being done. These factors for a few selected years are given in Table I.¹² The over-all lag of the United States, as well as

TABLE I. RESEARCH ACTIVITY IN GREAT BRITAIN, GERMANY, AND THE UNITED STATES IN METALLURGY, 1900-1928

| Country | Research factor | | | | |
|----------------------------|-----------------|------|------|------|------|
| | 1900 | 1910 | 1919 | 1923 | 1928 |
| United States: | | | | | |
| Total research | 3.26 | 2.28 | 3.00 | 1.98 | 3.23 |
| Fundamental research . . . | 0.64 | 0.51 | 0.66 | 0.55 | 0.66 |
| Great Britain: | | | | | |
| Total research | 4.87 | 5.94 | 5.55 | 4.81 | 8.93 |
| Fundamental research . . . | 1.32 | 1.46 | 1.53 | 1.01 | 1.52 |
| Germany: | | | | | |
| Total research | 4.57 | 4.57 | 3.86 | 5.70 | 7.17 |
| Fundamental research . . . | 1.13 | 1.43 | 0.80 | 1.50 | 1.49 |

the remarkably small amount of fundamental research activity, is clearly shown. At the same time, this country was forging ahead tremendously as far as production was concerned. The

¹² National Resources Planning Board, *op. cit.*, p. 158.

following figures tell the story of the tremendous expansion which took place between 1860 and 1900:

PRODUCTION IN AMERICA *

| Item | 1860 | 1900 |
|----------------------------------|-----------|-------------|
| Anthracite coal, short tons..... | 9,620,000 | 60,418,000 |
| Bituminous coal, short tons..... | 6,013,000 | 193,323,000 |
| Crude petroleum, barrels..... | 500,000 | 57,071,000 |
| Pig iron, long tons..... | * 751,000 | 13,621,000 |
| Crude steel, long tons..... | 10,000 | 10,640,000 |

* L. M. Hacker, *The Shaping of the American Tradition* (New York: Columbia University Press, 1947), Vol. 2, p. 692.

In a sense, industry did not seem willing to take the time needed for research. This expansion of production occupied the full energies of those in industry, although many improvements were made in actual production which today would fall into the category of research.

The recognition of the utility of trained personnel was slow in coming, and even as late as 1920 in industries where the results of research had been proved profitable, many executives looked askance at the thought of having scientists in business. It must be admitted that there were numerous examples of failure on the part of scientific personnel to solve industrial problems for which they had been hired. These failures, which were probably more common in the period before 1900 than we now realize, were due to improper selection of problems, inadequate personnel, and lack of adequate equipment. In other words, the resources had been badly chosen, and the result was research inefficiency. However, control of operations apparently was recognized as being necessary, and for this scientifically trained men were required. Chemists, metallurgists, and engineers were hired for the purpose of testing and checking raw materials, intermediates, and finished products. In some cases, consulting chemists were utilized. Bartlett notes that Arthur D. Little, whose laboratories were founded in 1886, reported that the testing of sugar provided most of the work of the commercial

chemists in Boston at the time. (A raw sugar analysis was made for 75 cents!) This was by no means research work, although it appears that the control technician was expected to answer questions relating to process problems on a moment's notice, unequivocally, and at no extra pay. However, the control chemist or test engineer often was able to indicate where research was necessary or likely to be useful; the way was definitely being prepared for the developments to follow.

It would appear that the usual early reasons for undertaking research in industry were personal, fortuitous, or essential. In other words, there were industries where the owners or managers were themselves interested in the scientific method and utilized it for their work. In others, a chance discovery with subsequent amalgamation of scientist and industry led to the organization of research. In still others, there was no way of establishing a manufacturing enterprise without recourse to the methods of science.

Authoritarianism in Research

In general, however, there was no recognition of any need for planned research. In concordance with the generally accepted mechanistic philosophy of the time, the solution of a problem was, in a sense, visualized by the industrialist as more or less final; *i.e.*, once a process or product had been established, it was considered the complete and immutable answer to the particular question, and the contemplation of changes was pronounced heresy. We would advance the thesis that *no one concept has or will handicap research in industry or elsewhere as much as has authoritarianism, within or outside the body of science*. The history of industry (and science) is replete with examples of authoritative pronouncements as to the future with respect to practices of a given time and the possibilities of further advances. These have usually been wrong.

An example in biological science is the controversy in the late nineteenth century between the great German biochemist, Hoppe-Seyler, who worked out the relations of hemoglobin in the blood, and MacMunn who discovered the existence of substances resembling hemoglobin in the tissues (*histohematin* and *myohematin*).

Hoppe-Seyler said that MacMunn's substances were simply decomposition products of the hemoglobin of the blood. MacMunn defended himself: he had shown in his very first paper that his substances were present in the tissue of insects, which have no hemoglobin in their blood. This might have seemed conclusive, but Hoppe-Seyler refused to consider the evidence from insects. He simply printed a note alongside MacMunn's last paper saying that he considered all further discussion . . . superfluous. . . . Little more was heard of histohematin, myohematin, or MacMunn. . . . It is strange to think that if MacMunn had not been crushed by Hoppe-Seyler, we should probably have had this knowledge nearly forty years sooner. A useful lesson can be learned from the sad story: under no circumstances must research be controlled by authority. It is true that Hoppe-Seyler had no legal authority, such as one scientist has over another in a totalitarian state; yet his influence was sufficient to retard by several decades the investigation of one of the most fundamental problems of life.¹³

No more striking example can be given than that cited by Bichowsky (which he advocates be placed "under the glass top of the desk of every executive and chief engineer") relating to the arguments by which the State Engineer of New York proved conclusively in 1850 that the railroad could never compete with the canal:

Canals . . . are facts; railroads are theories, and are opposed to the habits and feelings of our people, for they create monopolies in transportation. A farmer cannot own railroad wagons. But for a few hundred dollars he can buy a boat, or with the help of his hands can build one to carry twenty-five tons. To move such a load by railroad would require eight carriages and a locomotive costing \$4,000. Into his boat the farmer can put an assorted cargo of flour . . . [etc.], draw it to market with his own horses, sell it at any village on the way and bring it back loaded with what he pleases. Does anybody suppose railroads will take on loads offered anywhere along the line? No, indeed! The farmer must haul them to the stopping places. Canals will carry livestock, hay, firewood, large timbers for ships, building boards, and planks. Railroads cannot do this. . . . But [that] such a railroad as the Baltimore and Ohio ten times as long [as the longest then in operation] running through a rough, wild and sparsely

¹³ C. E. K. Mees, *The Path of Science* (New York: John Wiley & Sons, Inc., 1946), pp. 170f. For a more general discussion of the subject of authoritarianism and environment, see J. Frank, "The Place of the Expert in a Democratic Society," *Philosophy of Science*, January, 1949, pp. 3f.

inhabited country with great difficulties of construction to overcome should ever compete with a canal of the same length as the Chesapeake and Ohio surpasses probability.

If steam locomotives were used it would be necessary to have water-boiling stations every six or seven miles to furnish the engines with tanks of boiling water for a supply of cold water would stop the generation of steam and stop the train.

Rails would be broken by passing teamsters wantonly . . . or from spite . . . and would snap [in the winter] . . . as a train passed over them.¹⁴

This was reasoning with cold, hard facts, as they existed at the time, and, indeed, all these difficulties were present. Yet, these authoritative conclusions could not have been more wrong as predictions of the future. *A distinction must be drawn between a diagnosis of existing obstacles in the way of achieving a goal (and this is what the industrialists had to learn before research gained a foothold in industry) and a prognosis of the feasibility of overcoming those obstacles by systematic research.* Both factors are necessary, as we shall see, in the rational planning of industrial research, but they must not be confused.

Industrial Development

The early development of organized research in American industry is exemplified by the work of such companies as E. I. duPont de Nemours, General Electric, Standard Oil of Indiana, Parke, Davis & Company, and those companies built on the efforts of individual investigators such as Hall (the Aluminum Corporation), Bakeland (bakelite), Hyatt (celluloid), and Acheson (carborundum). There are many others which could be named, but the general pattern is basically the same.

The independent investigators were marked, perhaps, by less formal scientific training but with unswerving determination to overcome all obstacles in attaining their goal in whatever way possible. The extant records are generally those of the successful individuals; for every one who achieved his objective, there must have been many others who tried and failed. In general,

¹⁴ F. R. Bichowsky, *Industrial Research* (Brooklyn: Chemical Publishing Company, Inc., 1942), pp. 3ff., quoting McMaster, *History of the People in the United States*.

these men were marked by a comprehension of the possibility of creating a new product or a new process and an understanding of the general nature of the problems involved. Where successful, they were able to found new industries based on their results. Having been created in an atmosphere of research and problem solving, these industries maintained from the beginning an attitude of respect for the usefulness of systematic research. Their success had a profound influence on the remainder of American industry.

The established companies, such as those mentioned above, were usually interested primarily in the control of quality and secondarily in the improvement of methods or products. In E. I. duPont, for example, the scientific tradition was firmly grounded from the start by its founder, who had studied chemistry under Lavoisier, and was devoted largely to the improvement of chemical methods. The laboratories of the Standard Oil Company of Indiana were an outgrowth of the hiring of Dr. Burton to investigate the Frasch desulfurization process. For many years his laboratory was used strictly for analytical work and quality control, although inevitably it grew to the development of processes and then to research. General Electric was mentioned previously. By the very nature of that industry, research in product improvement was required. Companies such as Parke, Davis were active from their early beginnings in the standardization of their products (in this instance, drugs) by laboratory methods, and the development of improved processes to make these products more economically.¹⁵

The development of industrial research was thus proceeding in two general directions: (1) The independent investigators were questing new products or new methods of manufacturing established products, and (2) the companies who were utilizing a systematic methodology were interested mainly in *quality control* of their products, and thence in *improvements* in product quality and methods of production. As competition increased and the educational resources began to supply more technically trained men, these two types of motivation conjoined, and the research process was established as an integral part of those industries in which the scientific method had been traditionally

¹⁵ Cf. National Resources Planning Board, *op. cit.*, pp. 42-75.

utilized. With the First World War, the achievements of research were well known, and industrial research entered on a sound and continuous period of expansion. Naturally enough, the greatest part of this expansion was in those areas where its economic utility had been clearly demonstrated.

Other Motivations for Industrial Research

Industrial research motivations have been analyzed and expanded since these early developments, and a typical breakdown follows:

An industry should turn to research when it wishes to accomplish one or more of the following things:

1. Anticipate and prevent troubles.
2. Cure existing troubles and nuisances.
3. Reduce the cost of a product.
4. Increase the utility of a product through modifications or simply by finding new uses.
5. Reduce the consumer's operating or maintenance cost.
6. Develop new process material or products.
7. Improve the quality of existing materials or products.
8. Bring about better standardization.
9. Amass technical information leading to a better understanding of a product.
10. Contribute to the common store of general knowledge with the ultimate motive of increased markets through raised standards of living.¹⁶

It should be apparent that these objectives all fall into two general categories which should eventually be reflected in the profits of an industrial enterprise: (1) *increased sales of old products*, or *sales of new products*, and (2) *decreased production costs*. Obviously all enterprises should be desirous of improving their business from these two standpoints, but not all turn to research to accomplish these purposes. These motives are quite general and include all industrial research activities which concur with our definition. Where research is not utilized, the reasons may be found in one or more of the following conditions: (1)

¹⁶ F. Godwin, "To Our Sponsor," *Bulletin of Armour Research Foundation*, p. 9, Chicago, 1945.

ignorance and mutual antagonism of scientists and industry, (2) the lack of sufficient economic motivation in the total environment, (3) the low likelihood of success or feasibility in overcoming the possible problems, and (4) the lack of sufficient resources of personnel and method. Each of these should be considered in the outlining of a rational research program in a particular enterprise, as we shall discuss in the following chapters. They involve an understanding of the problems confronting an industrial organization, and the compatibility of these problems with the resources economically available to resolve them.

Even industrial research studies directed at the solution of problems not immediately connected with new or improved products or processes are undertaken with the anticipation of future gain. Thus, a company manufacturing chemical process mixers might study, or finance the study of, the hydrodynamics of viscous fluids under various conditions in the hope that increased knowledge would enable it to sell more mixers or improve the ones manufactured. The influence of the desire for industrial and community prestige also should not be overlooked as a motive for this type of industrial research. An example of the long-range results of such work may be found in the development of nylon by E. I. duPont after several years of research directed toward the solution of problems in the field of high polymers, such as those found in rubber, cellulose, and resins. Knowledge of the mechanism of polymerization led to the development of several high polymer materials which could be spun into fiber form in the molten state. With these facts at hand, an intensive program was undertaken to develop such a material with sufficiently adequate physical properties to make it competitive and economically feasible of manufacture. As a result of this work, a polyamide was chosen and a process developed for its manufacture. Nylon has repaid duPont manyfold for the quite high costs of the researches involved in its evolution.

A recent survey made by the National Association of Manufacturers points out that, of 441 companies answering a question as to the principal objectives of their research program,

40% reported that the principal objective of their research program is the improvement of existing products . . . [and the same percentage] said it was the development of new items. . . . However, many stated that they believe the development of new products is of greater importance to the long-term growth of the company.

Twelve percent (53) of the companies stated that improved manufacturing procedures is the principal objective of their research program; 5% (23), new uses for existing products; 2% (9), the salvage of waste material and 1% (6), the production of better raw materials.¹⁷

Apparently, the strongest motives continue to be those which offer the optimum opportunity for the greatest net economic advantage to the enterprise.

One other factor in industrial research motivation which should not be overlooked is the source, or the environmental background, of the administrators and directors of the activity. In the description of the development period of industrial research we noted that the individual investigators were often lacking in scientific education. The addition of research personnel to established enterprises, on the other hand, was accomplished largely by the hiring of academic personnel from institutions of learning. These were able to bring systematic methodology and the scientific approach quite rapidly into the companies which they entered, whereas in the former cases (although the companies were often successful), it proved more difficult to shake off the trial-and-error attitude of the individual founders. A number of firms which started with what appeared very promising futures were unable to maintain their competitive position because of this handicap. This was largely a case of not understanding what the systematic as opposed to the unorganized approach could accomplish. With the evolution of industrial research there has grown up a body of men, trained in the methods of science, with experience in industry, who are more or less competent to direct or carry out research in accordance with the recognized objectives of the particular enterprise. We have noted that the manner in which the individual was totally trained would affect the methods he would use to carry out re-

¹⁷ National Association of Manufacturers, *Trends in Industrial Research and Patent Practices* (New York: National Association of Manufacturers, 1948).

search. This implies that there are circumstances in which the research administrator, with only industrial experience and thoroughly imbued with the motives of industrial research, could cause, through lack of a broad viewpoint, the work of his particular laboratory to be inefficient. This question, of course, has a bearing upon the training and selection of research administrators.

The Magnitude of Industrial Research

The stake which the industrial economy has in the results of research is clearly indicated by the following estimate of some years ago of percentage of earnings accruing to various industries *from the discoveries of science*. These estimates are at best rough but, as given by the *Encyclopaedia Britannica*, indicate those areas which are economically founded upon the work of the scientist and engineer in recent times. Of course, not all of this stems from *research* as such, but future development must come from this source.

INCOME DERIVED FROM SCIENTIFIC DISCOVERIES

| | <i>Per cent</i> |
|---------------------|---------------------|
| Farms..... | 80 |
| Forests..... | 50 |
| Fish..... | 10 |
| Mines..... | 95 |
| Transportation..... | 100 |
| Manufacturing..... | 90 |
| Commerce..... | 85 |

The status of the research process in American industry today is clearly indicated by the recent rapid growth of the expenditures for it, and of the number of personnel engaged in it. Personnel and expenditures have continued to increase over the past few years, as is indicated in Figures 1 and 2. Figure 1 shows that the number of professional and technical personnel in industrial research has increased from less than 10,000 in 1920 to over 95,000 in 1946. Figure 2 indicates the increase in industrial research expenditures from 116 million dollars in 1930 to approximately 500 million in 1947. It is estimated that the total, including government, institutes, universities, and industry, was over a billion dollars in 1947. While these figures are necessarily only

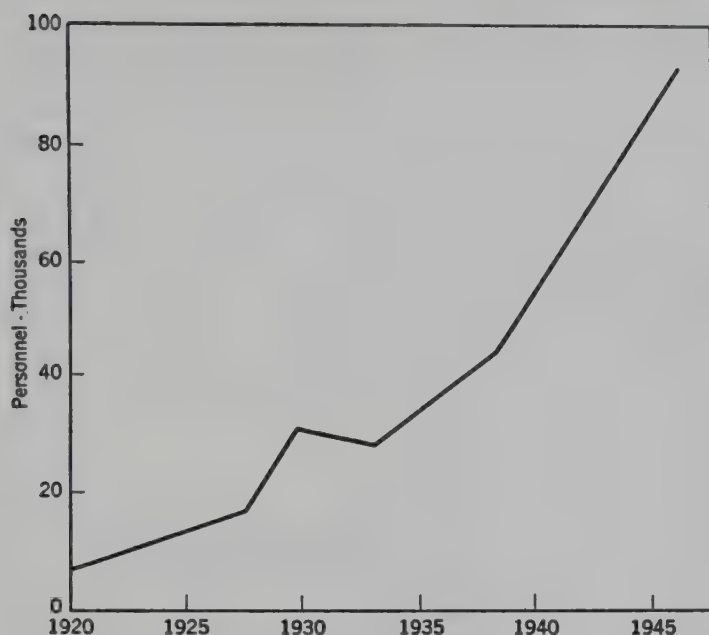


FIG. 1. Professional and technical personnel employed in industrial research, 1920–1945. [Data for 1920–1940 from *National Research Council, Research, A National Resource, Vol. 2* (Washington: U.S. Government Printing Office, 1940), p. 174; for 1940–1945 from *National Research Council, Industrial Research Laboratories Bull. 113*, 8th ed. (Washington, U.S. Government Printing Office, 1946).]

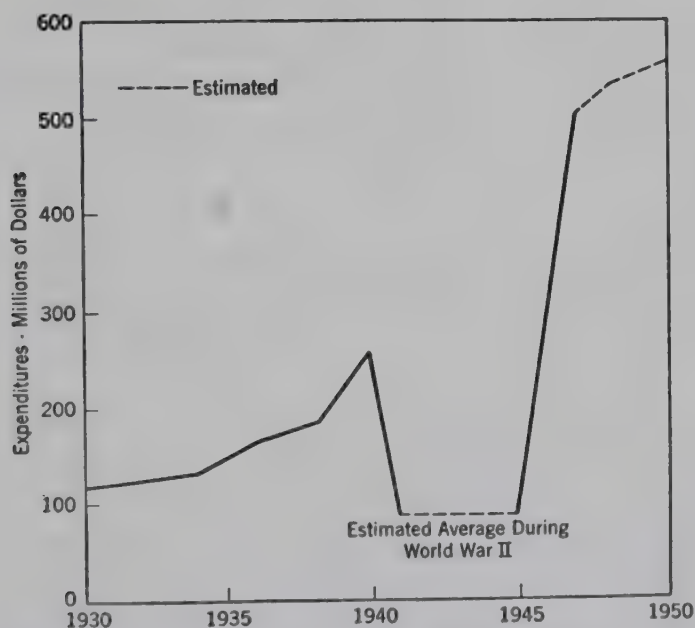


FIG. 2. Industrial research expenditures, 1930–1950. [Data from *John R. Steelman, Science and Public Policy, Vol. 1* (Washington: U.S. Government Printing Office), pp. 10ff.]

approximate¹⁸ and are not adjusted for variations in the purchasing power of the dollar, they are adequate to indicate the increasing magnitude of the research operation. Of 983 companies reporting in the National Association of Manufacturers

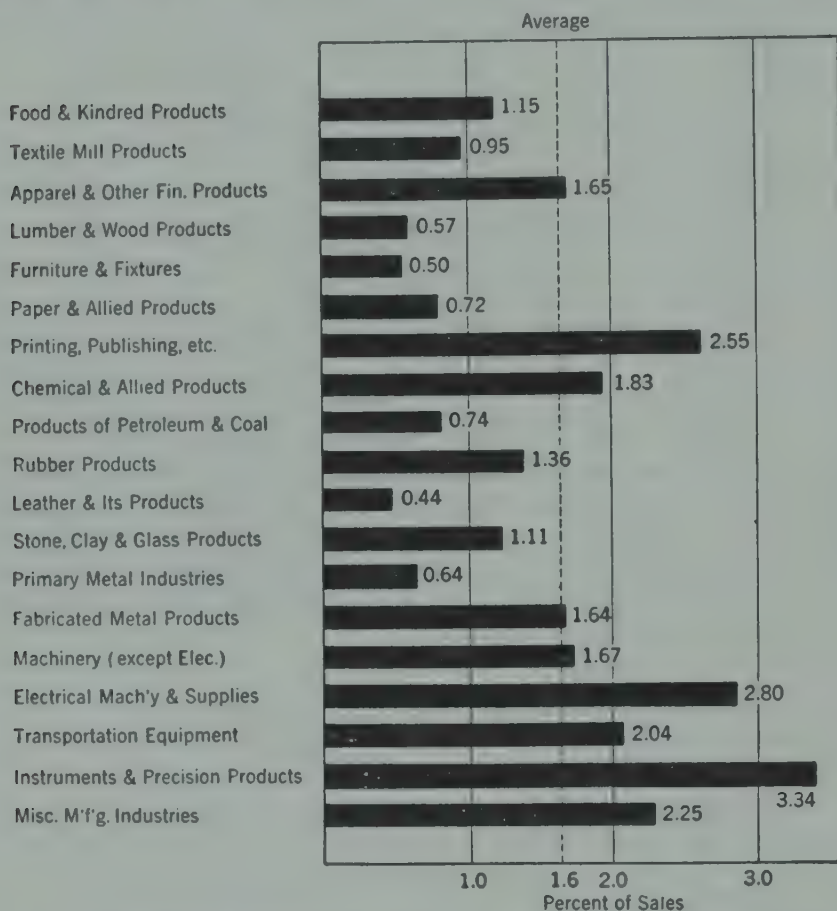


FIG. 3. Percentages of sales expended for industrial research by companies that have a research organization, 1947. [*National Association of Manufacturers, Trends in Industrial Research and Patent Practices* (New York: 1948), p. 78.]

survey, 750 reported having a research program. However, it is true that the bulk of the industrial laboratories are in the large corporations. There are approximately 17,000 manufacturing firms having annual sales of \$500,000 or more, and of these, perhaps 10 per cent have research and development facilities.

¹⁸ There is no standard for reporting research expenditures, and accounting procedures vary widely. Therefore, any figures must be at best an approximation.

On the other hand, in a list of America's 100 largest industrial corporations, there is not one which does not maintain a research staff and laboratory.

We have estimated elsewhere that somewhere between 1 and 2 per cent is a fair average for research expenditures in terms of

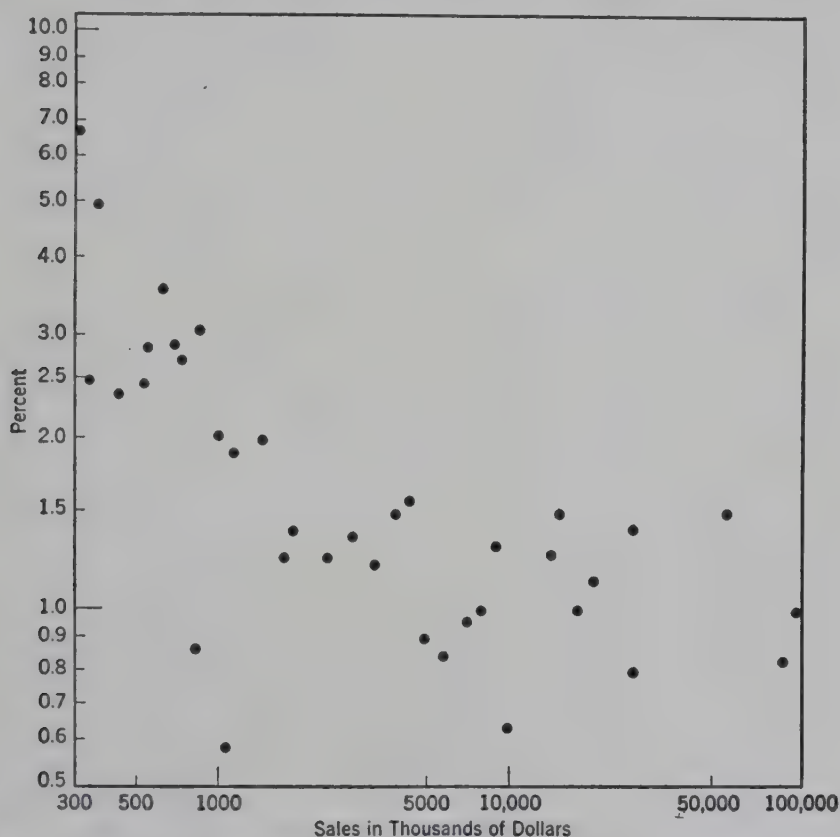


FIG. 4. Average ratio of research expenditures to sales by companies that have a research program. [Averages plotted at mid-points of sales ranges. Data from 750 companies. *National Association of Manufacturers, Trends in Industrial Research and Patent Practices* (New York: 1948), p. 75.]

sales for manufacturing enterprises in general. This average may vary in individual industries from nearly 4 per cent in the precision manufacturing field to less than one-half of 1 per cent in leather and leather goods, as reported by the survey mentioned above. These averages should be taken only as indicative, since they are subject to the same inaccuracies previously noted. Figure 3 shows these percentages for a number of different industries in 1947. The concentration in the traditional chemical field is

indicated by the fact that of the \$207,384,188 reported as having been expended in this survey, some 40 per cent is in the chemical, fuel, and rubber industries.

Expenditures as a percentage of sales in terms of the size of the corporate unit for companies that do have a research program are shown in Figure 4. Quite clearly, the smaller units find it

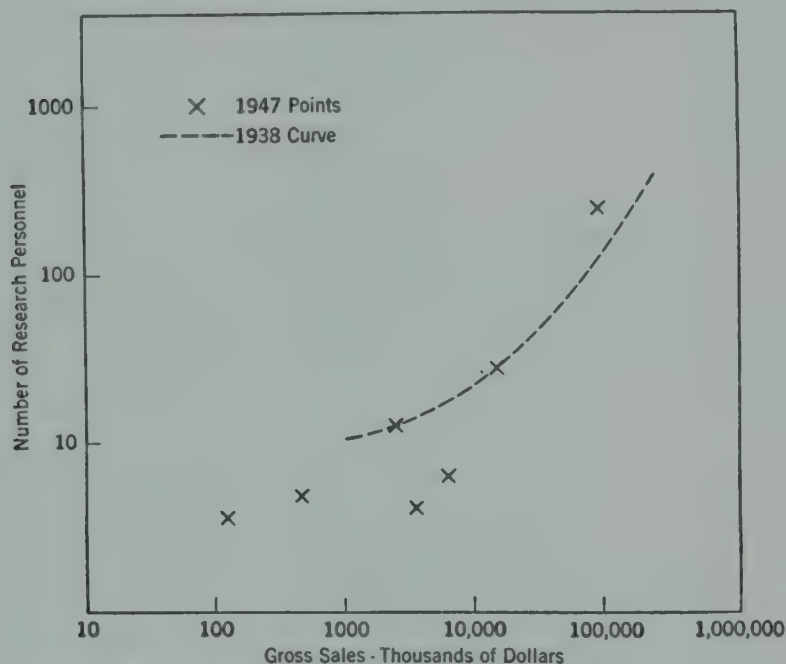


FIG. 5. Average size of research staffs maintained by corporations doing research, according to gross sales. 1938 and 1947. [Data for 1938 from *National Research Council, Research, A National Resource, Vol. 2* (Washington: U.S. Government Printing Office, 1940). Data for 1948 from *National Association of Manufacturers, Trends in Industrial Research and Patent Practices* (New York: 1948).]

necessary to expend a greater proportion of income than the larger ones in order to obtain worth-while results from a research program. The critical point appears to occur in the neighborhood of \$10,000,000 in sales. Above this amount of sales, it would seem that a fairly constant average percentage maintains a suitable research program.

Figure 5 shows the number of research personnel employed in terms of sales for a National Research Council survey of 1938 and for the National Association of Manufacturers survey of 1948. Apparently, there has been little significant change in the

relative utilization of research personnel by industrial enterprises based on corporate size, during the intervening 10 years. Since these were years of expansion and growth, in which many of the firms originally reporting will have increased their sales considerably, perhaps there is some constancy in these ratios. Of course, these figures do not reflect the use of the numerous outside laboratories by the reporting firms, nor is the inflated dollar taken into account, which would result in a higher cost per researcher employed. It may be that the use of research personnel is based, not so much on need, as on some social growth factor within a given research department. As sales increase, so do the research organizations increase, within acceptable limits.

Problems of Industrial Research

The tremendous growth of research in industry generally in the years following the Second World War is not necessarily a happy one. As Dr. Houston has pointed out,

Ten years ago the difficulty was to persuade those with the responsibility of allocating funds to provide enough for research. . . . Today the situation tends to be the opposite. Both industrial and government funds are being poured into research at such a rate that the limiting factor is a shortage of trained personnel. This creates other problems of administration, for nowhere is it easier to waste money than in this kind of activity. A research man, lacking insight and imagination, can spend just as much money, if not more, than a man of genius, and I sometimes fear that the present high regard in which research is held may eventually suffer from too lavish an expenditure of funds in the wrong places.¹⁹

This is all too true and, as we have said, the expenditure of resources in solving problems should be commensurate with the *feasibility and necessity* of their solution in a given environmental situation. It is hoped that the principles of organization and administration which will be dealt with in the chapters to follow will assist those responsible for the control and operation of industrial research in maintaining and improving its status in industry. There is no doubt that large sums are being wasted

¹⁹ W. V. Houston, "Research and Industry," address at the dedication of a new synthetic glycerine plant in Houston, Tex., *Chemistry*, Vol. 22 (November, 1948), p. 7.

through insufficient understanding and application of sound knowledge of the fundamental elements of the process. *Industrial research is young, it has grown extremely rapidly—it is a necessary as well as useful adjunct to the manufacturing process, if properly applied and administered.*

The research situation, as well as any other, is susceptible to rational planning, based on the elements of the problems involved. However, in dealing with the production of ideas by creative personnel, a business enterprise finds itself faced particularly with the problem of developing an integrated philosophy. In the first place, the decision to enter research is not so simple as, for example, the problem of whether or not to have an accounting department. The one is required, the other a matter of choice. If the decision is based on the anticipation of increased profits, and these are not forthcoming, what then? Intangible value judgments may, nevertheless, indicate research to be worth while. Methods of operation are not clear-cut and are even now subject to much dispute. The area of problem choice is a wide one, and *without a philosophy of research operation, harmonious with the external and internal environment, in combination with suitable resources, industrial research cannot be successful.*

CHAPTER VI

RESEARCH PROJECTS AND PROGRAMS

The Start of a Research Program

The genesis of industrial research in a particular company is (or should be) the selection of a project consisting of one or more problems of interest to the enterprise. The solutions to these problems, if achieved, are generally expected to result in some net advantage to the company. The organization of the research unit, its resources, techniques, and methodology should depend, as we have indicated, upon analyses of the problems.

Management's Responsibility

A program for research may be defined as comprising all the projects selected during a given period of time. Apparent as the existence of problems to include in such a program may seem to be, the choice of those to be presented hopefully to an industrial research group for solution is not simple or uniquely defined. In the final analysis, it is the top or controlling management of an enterprise which must assume the ultimate responsibility for this choice. Certain criteria upon which to base a critical and rational analysis of the proposals under consideration may be formulated. Their use will aid management in choosing wisely and thereby increase the efficiency of the research process in industry. The final decisions must be reached by means of the judgment of those who determine policy for the enterprise, since *completely* determinative metrical formulations of desirability and feasibility will not be available. However, the optimum decisions which can be made are likely to be determined by the rationality, pertinency, and accuracy of the data in the possession of policy-making executives. These data, which will be discussed in some detail, will serve equally well in arriving at answers to the questions:

1. Shall we, in a particular company, embark upon a research program?
2. What should a program comprise?

A Research Program—or Not?

In the first instance, the decision to enter research is one which should not be undertaken lightly. Clearly, this question is closely connected with that of the choice of a program and much the same criteria should and do apply. The establishment of research activity is a problem in planning, and before plans can be made efficiently, as much pertinent data as possible must be available. The fact that research is successful in other areas of the industrial economy is of only minor importance in comparison to *significant* information concerning the specific enterprise. Thus, all concerns should not necessarily have a research program: such programs may not be necessary, feasible, or economically sound. Wishful thinking based on opinion, desires, and the success of others is not sufficient to ensure success in this very difficult field of activity. Recognition of the elements which the problems posed for solution contain and an understanding of the resources which will be required for their solution are far more important. Inadequate comprehension of the basic requirements for successful research can only rarely be overcome by lavish financial expenditures.

The Consequences of Insufficient Consideration

The textile industry has been cited as having lagged in undertaking research and, as a consequence, in building up a body of scientifically trained creative personnel with the requisite experience to solve some of its problems. As an example of the difficulty of undertaking research without prior visualization of the elements of a problem, the experience of one very large corporation may be described. This company had not done any previous work in the textile machinery field, but based on (1) the extremely high demand at the end of the Second World War, (2) the small number of firms in the field, (3) the lack of desirable improvements in the product available, and (4) its own successful tradition in engineering and technology, decided to

design and produce an improved unit of basic equipment for the industry. The extent of the preliminary investigations is not known, but in any event the market research was probably sound. The field must have appeared extremely tempting, particularly since this was a giant of industry compared to the other manufacturers of this equipment, and the potential market was quite large. Apparently it was decided not to study the fundamental problems of the process. By the use of the most modern techniques, it was hoped to improve the units then being manufactured.

Personnel familiar with the machinery were employed and a large-scale program set in motion. As the new unit progressed, the major advantages to be offered apparently were to be interchangeable subassemblies of the operating components and approximately a 25 per cent increase in production rates. It appears that production facilities were arranged *before* user trials and analyses were completed. Field testing of some 200 units was undertaken as the development proceeded, and before they were finished it was clear that serious deficiencies existed in the equipment. Among these was the difficulty of maintaining the necessary alignment and tolerances under actual production conditions so as to take advantage of the interchangeable subassemblies and higher speeds. Eventually all the trial units were withdrawn and, at least temporarily, no more were offered to the market. A conservative estimate of the cost of this development program is in the neighborhood of 2 million dollars, the only returns from which up to this point were a loss of prestige and a great deal of sad experience. Admittedly, industrial research is a gamble in many cases, and the larger the risks, perhaps the greater the potential return. However, we feel certain that this organization did not visualize the program as being particularly risky, and its failure probably came as a great surprise and shock.

The Importance of Considering More Than One Factor

In this particular instance, the error lay in underestimating the resources required to undertake the basic program, and in entering manufacturing with what were, to all intents and purposes, only unproved minor improvements over previous practice. In

a sense, the decision to do this was influenced by the market conditions which existed at the time, and the desire to reach this market before much of the demand was filled by companies already in the field. This may have been a sound decision from a sales standpoint but was not sufficient to ensure the success of the project. This example illustrates the necessity for consideration of more than one or two factors involved in the choice of a project or program, before a decision is reached. In this case, if more complete data had been available to management, quite a different outcome might have been possible. A comprehension of the lack of scientific data concerning the characteristics and requirements of the process under consideration should have indicated the need for a more complete exploratory program. It is likely that this would have led to a further program for the study of the fundamental elements, taking more time perhaps, but costing a great deal less than was actually expended. Such a decision might have led eventually to a much sounder unit, or to an abandonment of the project long before production facilities were provided.

Basing research judgment upon a single factor—the market in this case—has the effect of placing bounding or limiting conditions upon the process, which considerably increase the risks involved. The requisite data upon which to base the decision to enter research, or approve (or reject) a project or program, include, in addition to financial considerations and market analyses, information as to *areas of competition*, *areas open to research attack*, *feasibility of solving problems* in the desirable areas, and *estimates of the resources* which will be required. These will be discussed in detail. It should be stressed that it is the *complete* analysis which is important; in individual cases particular factors may be predominant, but since there is no general rule, we may say that the more information available concerning *all* the criteria, and the more accurate this information is, the more likely is the success of the research program.

FINANCIAL CONSIDERATIONS

Among the basic criteria which should be given consideration are the size of the company and its financial status. Clearly these will have a bearing upon the research program to be under-

taken. There are two factors which are of importance in this connection: (1) the amount of working capital which can be utilized for research purposes and (2) the amount of investment capital which may be available, from the company's surplus or from other sources, to utilize the *results* of the research work.

In the first instance, the fundamental data are those of sound financial operation which apply to the enterprise as a whole. No set rules can be established, although there is a tendency to assume that approximately 2 per cent of sales income is a satisfactory proportion to be appropriated for research. This is an average, and in common with most averages is, at best, indicative of a general condition.¹ The research budget should not be determined in advance of the establishment of the research program, but the expenditures which are potentially available will be limited by the working capital of the enterprise and the requirements of other departments. Therefore, the decision of those responsible for financial policy as to the upper limit which can be safely expended in a given period, if a program is pursued, is one of the criteria which should be determined and made available for consideration in this connection. These data should be assembled with the idea in mind that the returns on these expenditures will be deferred and may require the investment of capital funds prior to their realization. The percentage of sales income thus indicated will vary then, depending upon the financial status and policy of the individual enterprise. The amount derived in this manner will not be that which necessarily will be expended, but should be dealt with for what it is—a safe upper limit on funds to be provided for research from the working capital of the enterprise. As such it can play an important and useful role in the over-all consideration of the program.

The desirability of establishing a limit on expenditures prior to deciding on the research program does not mean that the small company should engage in what O'Leary describes as "haphazard short range research problems which could easily

¹ National Association of Manufacturers, *Trends in Industrial Research and Patent Practices* (New York: National Association of Manufacturers, 1948), p. 78. An over-all average of the companies reporting in this survey (983) indicates that 1.60 per cent of sales was expended for research in 1947. This compares with 1.87 per cent in 1946. See Figure 3.

be spotted at once as unprofitable ventures by competent technical men,"² or that the large company, with greater sums available, should indulge in impractical long-range ventures. It should be recognized that the majority of research projects, if successful, will require an investment for realization of the profits to be gained by the utilization of the predicted results. It is true that certain types of solutions to research problems do not require additional expenditures to be fruitful, but these are in a minority.³ Generally, the development of new products or processes requires investment in facilities to produce these products or to utilize the processes, and improvements in either of these categories usually do also.

As Bichowsky says, "The first of these facts [barring successful exploitation of research in industry] is that the creation of wealth out of these potential values [the results of research] requires capital investment larger than the wealth created." By this he means that the new product or process may create demands which the originating enterprise cannot fill, and competition will obtain the necessary additional capital to bridge the gap. He states further, "The upper limit [of research expenditures] . . . depends on the fact that unrestrained research can soon invent so many things to manufacture and sell as to be quite beyond the capacity of an organization with limited capitalization, plant and sales outlet."⁴

Hence, the second criterion with regard to company size and financial status is its situation with reference to obtaining investment capital. The data utilized here may be either general or specific; *i.e.*, it may relate to the general condition of the company's surplus, the anticipated utilization of it, and the situation with regard to obtaining additional long-term capital in the form of loans, equity financing, etc. It may also include specific information as to process equipment which may have

² L. O'Leary, "Research in the Paint Industry—How and How Much," *Paint, Oil, and Chemical Review*, Oct. 2, 1947, p. 16.

³ In this classification are market research projects which indicate negative results, *i.e.*, that there is a poor market for the product under consideration. Also, research leading to the elimination of process components or the choice of improved raw materials may require no significant capital expenditures.

⁴ F. R. Bichowsky, *Industrial Research* (Brooklyn: Chemical Publishing Company, Inc., 1942), pp. 17, 22.

been sufficiently depreciated, so that reserve funds are available for its replacement. Such information, when considered in conjunction with the other data pertinent to research activity, can be of great assistance in arriving at a rational program.

The writer recently investigated a bankrupt small electronics firm and found that neglect of these two factors by its management had been at least partly responsible for its failure. The company had employed several very competent physicists and electronics engineers and for approximately two years prior to its failure had been doing research on a technically sound and promising project. Research expenses were averaging almost 10 per cent on sales and creating quite a drain on working capital. As might be expected, the situation rapidly deteriorated and, by the time the development was ready to be exploited, the firm had no capital whatsoever to initiate production, and its credit position was so poor that it could not obtain funds elsewhere. Its only recourse was to sell the results of its research work, but before such a sale could be consummated it was in bankruptcy. Undoubtedly this was research mismanagement of the worst sort, and despite the fact that the laboratory was well equipped and staffed for the work to be undertaken, the research process was a handicap to this firm rather than a help. The recognition of sound financial considerations, in combination with the technical factors, in setting up the research program might have prevented the failure.

COMPETITIVE POSITION

Another factor of importance in passing judgment upon research projects and programs is that of competition. All enterprises, even those founded on strong monopolistic bases, are faced with competition in one form or another. It may be actual or potential, direct or indirect, and may vary in intensity with geographical and price factors, general economic conditions, etc. In order properly to evaluate proposed research projects and programs, information should be available as to the position of the company with respect to these factors. We are not referring to specific market research with respect to a new or improved product proposed in the research program. This would be a part of the actual work, and the need for such research

should be indicated by the over-all criteria we have been discussing. What we are concerned with here is market data of sufficient accuracy which will enable management, in its considerations of a research program, to correlate the proposals with those areas of the enterprise's activity where competition is actually or potentially a significant factor. Sales and market trends, competitive price policies and activity in the development of new products, patent situations, raw material positions, etc., should all be carefully evaluated in connection with a proposal.

It should be apparent by this time that *the research program of a particular enterprise is not a thing apart, to be developed from technical considerations alone, but an integral entity dependent upon all those factors which affect the remainder of the company.* Thus, the information concerning competition, which we have indicated as being necessary in order to reach the best possible decisions concerning the nature and extent of research activity, would also be required to set proper sales and production policies. It is for this reason that top management must assume the responsibility for the final evaluation of the research program.

NEED FOR COMPLETE MARKET AND SALES DATA

As an example of the utility of data in preventing serious errors of judgment in this regard, we may cite the case of one corporation which made a costly mistake through lack of appreciation of the importance of this factor. This company, faced with rising costs of a particular raw material in the postwar markets, embarked upon the development of a process for the production and use of a substitute. A large plant was eventually erected to turn out this material. Before it was in production, the price of the original material began to drop, and it was evident that it would approach, if not fall below, the point where it would be less expensive than the substitute. Since it was in many ways the better material, the result appeared to be that, either the company's competitors would have a price and quality advantage, or the company would have to abandon its investment in the new plant. It may be that a market evaluation, undertaken at the time the original decision was made, might not have indicated the far-reaching price drop. However, since

the only analysis consisted of the opinions of those who also made the decision, we may justly say that more adequate advance planning might have prevented this error.

ANALYSIS OF AREAS FOR POTENTIAL RESEARCH

In addition to the financial and competitive criteria, information as to the potential areas of research attack should also be made available to those charged with deciding upon a research program. "You have to recognize a possibility of improvement before you are in the slightest degree discontented with what exists."⁵ The other factors being favorable, research should be reserved for those areas where the highest potential net advantage to the company exists. In a small company, this consideration may limit the program to one project only. In the larger organization, it should indicate the relative emphasis to be placed upon various phases of research activity. In general, there are two major areas upon which attention should be focused: (1) products and (2) processes.

Analyses should be made of the company's products to determine their susceptibility to improvement, as well as the desirability of adding new products for manufacture and sale. These analyses should be made in general *economic* terms—specific technical feasibility is a separate criterion. *Changes* in products and processes may be desirable to (1) improve product performance, (2) cut production costs, (3) standardize parts, and (4) lower selling prices. In so far as products are concerned, these may be effected by (1) physical improvements in design or manufacturing process, (2) improvements in appearance and style, and (3) changes in materials. *New* products may be desirable to (1) make fuller use of present plant equipment, (2) make fuller use of present sales organization, and (3) diversify the product line. In the latter instance the economics of line simplification must be compared with the marketing insurance which is obtained by diversification. It should also be noted that adding new products will generally increase the areas of competition.

⁵ Sir E. R. Streat, "The Firm without a Research Department," *Mechanical World*, Mar. 29, 1946, p. 349.

In analyzing the area of new products, the following types should be distinguished: (1) products supplying well-defined wants, (2) products in which the use requirements are problematic, and (3) products which will definitely require the creation of new wants on the part of the consumers. Clearly, the *risks*, *areas of competition*, and *potential gains* are different in each of these cases.

Before consideration is given to any research program in the area of new or improved processes or manufacturing methods, there are two types of data which should be made available to top management. These should relate to all the particular enterprise's processes: (1) *the age of each process* and (2) *the cost of each process in terms of the final product manufacturing cost*. It should be apparent that the older processes and products probably offer the best opportunities for improvement, and that the more costly portions will offer the best opportunities for high returns from changes or improvements. Such information will enable management to make decisions with the over-all viewpoint in mind.

A third factor of interest, but concerning which data may not be available and which may require research in order to obtain the necessary information, is the effect of each of the processes upon the *quality* of the product. If information concerning these three factors is available, research projects involving process areas which affect neither the quality nor the cost of the product in a significant degree will be eliminated before even a preliminary investigation is authorized. It will also be found that the most profitable approach will be a coordinated over-all attack upon a process, rather than a segmental one, which will be able, at best, to lead to minor advantages. The point is that *research work should be reserved only for the processes that have the highest potential return*. Information as to the age of a process in conjunction with the amount of depreciation or obsolescence reserves available to replace the equipment is of importance in deciding whether or not to explore a particular area. If a majority of the equipment has been depreciated and if replacement funds are on hand (as they should be), research leading to a new process prior to the installation of new equipment may well be warranted.

An Example of the Necessity for Continuous Exploration of the Areas of Research

A large company recently withdrew from a well-established line of products which it had been manufacturing for many years. Shrinking markets and competition from other products played a large part in this particular case. In addition, the lack of significant process improvements over a period of years was also responsible. Methods and equipment were being used which had long since paid their way. Undoubtedly, as is too often the case, the specific reserves had been used for other purposes and might not have been available at the particular time when the decision to withdraw from the field was made. But even if it had been otherwise, new and improved processes in which to invest were not available; such research work as had been done was sporadic and haphazard. Despite the fact that the age of the process and equipment was well recognized and that the trends in the industry were clearly apparent, no coordinated attack had been made to evolve a new and less costly manufacturing process. The loss of potential profits was substantially due to the necessity for withdrawing from the field. Apparently this factor is often overlooked in the quest for maximum profits on outmoded and obsolescent manufacturing methods.

The Technological Feasibility of the Research Program

Another important criterion in considering the research program is the technological feasibility of the research attack in those areas where the economics are favorable. This can be only an estimate of possibilities and will require the best technical advice available. In the case discussed above, if the research and other technical personnel of the company had been consulted in previous years and had indicated that success in evolving an improved process was unlikely without an inordinate expenditure of time and funds, then the decision which was taken might have been the correct one. As a recent editorial puts it:

Several progressive industrial research laboratories are now exploring new techniques for setting the odds on research more in their favor. In these organizations, each new proposal is tested for its ultimate

business potential before any experimental work is begun. Each new development problem is viewed from all the possible ways of solving it. The welter of possibilities is sifted to eliminate all but the one or two alternatives that promise the greatest profit. Experimental work is reserved only for the processes that have the highest potential.⁶

Here, management must rely on the judgment of its research and technical staffs. The advice and information obtained from them should be as factual and impartial as possible. This is not the time for "selling" a specific project—*the object in furnishing top management with the information making up the criteria we have been discussing is to provide them with the requisite background for making sound, coordinated decisions with regard to research.* An unsound or prejudiced bias at this stage can do industrial research in a given organization more harm than good. The opportunity to "sell" a particular project should be given, but in connection with a later analysis of that project, and not of a company's over-all position. What is most desirable is a survey of the technical position of the enterprise's products and processes, and of the resources of men and materials available to it. This survey should be sufficiently broad so that it clearly indicates the strengths and weaknesses of the company with respect to the status of pertinent scientific information available. In other words, it is a *first step* in scientifically evaluating the elements which make up the obstacles or problems standing in the way of achievement of objectives or goals which may have been proposed in specific (or general) programs or projects. As we have previously noted, the broader the intelligences available to make this survey, the clearer and more useful will be the results. In the smaller companies particularly where the technical staffs are limited in scope, it may be desirable to utilize the knowledge of external technical consultants in order to obtain a more comprehensive picture. What top management should seek to realize from this technical advice is an understanding of the general feasibility of attack upon the areas of the company's activity most likely to bring a worthwhile net advantage to the enterprise.

⁶ "How to Increase Research Profits," an editorial, *Chemical Industries*, March, 1947, p. 421.

Research Planning

The criteria we have been discussing, (1) financial, (2) competitive and market positions, (3) economic analyses of products and processes, and (4) technical position of the enterprise's activities in relation to scientific knowledge and techniques available, are basic to any rational critique of proposed research projects or programs. *Research planning in industry should begin with an analysis of these factors on the part of management.* In other words, the attack should proceed from the general elements in the over-all situation to the more detailed analysis of specific proposals. Such planning methods are by no means universally used. We have seen examples of the danger inherent in proceeding with research programs which have been analyzed from narrow viewpoints, on the basis of wishful thinking or irrational opinions.

These criteria apply equally as well to the establishment of new research organizations in industry as to the evaluation of going research activities. The objective is that they be brought to bear upon the specific projects and programs which are either proposed or under way. From this information plans may be made, and the more accurate and pertinent the information, the more likely are the plans to be successful. With these plans, which will in general establish the personnel and material resources necessary to carry out the desired program, the research organization and budget may be rationally designed. The reverse procedure of establishing the organization and budget first, and then deciding on the program, is definitely inefficient in the light of any rational synthesis of research methodology. However, the objection may be raised, that once research activity is organized it will be relatively fixed, and that this planning procedure may not be applicable. This problem can be profitably resolved, and in a later section certain methods of organization will be proposed to provide necessary flexibility in the research setup. Use of these methods should permit the research group in industry to be guided by the principles outlined and to operate efficiently.

The analyses of the four factors which we have discussed have been considered in the light of an over-all program. They also should be applied to specific proposals for research projects, which should harmonize with the resulting plan if a consistent policy of efficient operation is to be pursued. In order to apply these principles to specific projects, it is necessary first to consider the general classes of industrial research activity and then the specific areas in which they may be applied.

Categories of Industrial Research Activity

The application of available research resources to a given project or program can vary widely in intensity, and a general classification may be established on this basis. If a concern has five (or five hundred) technical personnel all working toward one specific objective which is clearly defined, the work may be considered *intensive* research. Thus, the ill-fated electronics firm described previously was embarked upon a program of intensive research leading to the development of one particular item. On the other hand, if the personnel are split into a large number of groups, in proportion to the total available, each working intensively upon different, specifically defined problems, the over-all program may be termed *extensive*. If the problems are not clearly defined and if the objective is the investigation of feasibility and possibilities, the work may be considered to be *exploratory*, which generally is a form of extensive activity. Beyond the intensive stage, there lies the class of research in which *pilot or plant-scale trials* preparatory to commercial use of the results are conducted.

From exploratory work to intensive to the trial stage, the problems become more narrowly defined and the objectives more specific. It will also often be true that the work becomes more costly and the risks of error due to poor planning become greater. These errors will generally be those of commission, whereas those caused by poor planning of exploratory work will be those of omission. *The latter may be far more costly in the long run but may not be readily apparent.*

Whether research is extensive or intensive will depend in large measure upon the proportion of total research resources expended

upon projects with differing objectives. The decision as to which type is better for a given enterprise at a certain time can be made only in the light of the criteria discussed above. There is no a priori rule which can be made to apply, and the situation will not remain static. The size of the firm is not necessarily the deciding factor, as is commonly felt. The areas which will produce the most worth-while results from an economic standpoint, and are the most feasible technologically, may be those requiring a small proportion of the research resources, in a given instance. For example, an electrical manufacturing company, whose gross sales income was approximately 8 million dollars in 1947, employed some 40 technical personnel in research who were (during that year) engaged in the solution of some 80 specific problems. These problems were quite varied and related both to processes and products. The net advantage to the enterprise resulting from the activities of the research department was estimated to be well over 1 million dollars annually. On the other side of the picture, there are larger companies which have successfully thrown their entire research resources into the solution of one specific problem, the results paying handsome dividends. Of course, the reverse situation also exists, and examples could be cited showing that both extensive and intensive programs have been failures, in large and small concerns. *This would indicate that there is no intrinsic virtue in either of these approaches, and that basically success or failure stems from the combination of proper planning with adequate resources.*

On the other hand, the utilization of exploratory research, intensive research, and trials, as stages in the program, is definitely advantageous. The decision to pursue intensive research in a given program should be preceded by as comprehensive an exploratory investigation as the available resources and the technical aspects of the problem permit. Just as adequate information can prevent the waste of these resources on unpromising explorations, so the explorations themselves can serve as a guide to those avenues along which intensive research may be followed with profit. Exploratory work should start with the proposal of a project, and as we shall note again below, no proposal can be considered complete without an appraisal of the possible avenues of solution.

Specific Areas for Research Projects

PROCESS RESEARCH

Each of these measures of activity may be applied to the specific *areas* of industrial research. These areas include (1) investigations of processes, (2) products, (3) markets, (4) raw materials, (5) waste utilization, and (6) organizational relationships. A project or program *may* require research work in any or all these fields in order to be carried to a satisfactory solution. It should be apparent that the more completely rounded and coordinated the solution to a particular problem is, from all these aspects, the greater will be the opportunities to realize worth-while profits.

In process research the objective should be "not only to find 'a way,' but to find 'the one best way' of producing new [or old] products." The *best* way may include specific requirements as to quality, cost of plant, operational and maintenance economies, etc. Process research includes problems relating to over-all process areas and intraprocess areas. *Over-all process research may be defined as comprising investigations of complete processes or of the interrelationships of particular manufacturing processes with a view to improvement, integration, or the development of new methods.* Over-all process research is often a fertile field for profitable work and may be considered as the internal area for maintaining a dynamic manufacturing structure. It is apt to require a larger research organization and to be more costly than intraprocess work, but, if properly planned in relation to the other significant factors, can offer larger returns on the research investment.

Intraprocess areas of research activity are those in which investigations of a specific component of a process are carried on. They may include the development of an improved processing machine or changes in a part of a process to enhance its efficiency. The choice of a particular unit for investigation is in many ways more difficult than the choice of an entire process, and perhaps more important. It is quite possible to expend a great deal of effort on a particular process component with only minor savings, when the same amount of resources might be

able to improve or change the entire process with major results. On the other hand, the resources required for this type of work may not be so great, and if the costly or otherwise important elements of the manufacturing procedure are selected for research, the results may be quite profitable.

PRODUCT RESEARCH

Product research, leading to either new or improved products, cannot be completely distinguished, in practice, from process investigations. A new process may either be required for the production of a new product or in itself give rise to one. However, if it is recognized that the two more often than not are part of the same research project, we may discuss some of the aspects of product research separately. Either the improvement of old products or the development of new ones is preceded by a recognition of a demand or of the possibility of creating a demand. The study of the characteristics of this demand and the prediction of future trends are properly the subjects of market research. Development of new products without adequate market research is considered extremely hazardous, and even changes in design should be preceded by some investigation of user acceptance.

With reference to the product itself, research may be carried on to improve its performance characteristics, by direct or indirect modifications. A direct modification might come about through design research, leading to such changes as substitute materials, improved purity, or higher efficiencies. Indirect modifications might follow investigations into improved methods of product use, or better materials for use by the consumer *with* the product, as investigations of plasticizers by the manufacturers of plastics, or of yeasts by the manufacturers of flour. A great deal of such product research consists of technical assistance and service to the enterprise's customers. It should be directed toward improving the sales position of the company, whereas process research may be devoted largely to improving the cost position. These categories should not be considered as being mutually exclusive, but they are convenient in analyzing and classifying a company's research activities.

MARKET RESEARCH

Market research, although often considered an adjunct to the sales organization, is, as the name implies, a well-defined research activity. It has been regarded as such more by the chemical industry than by any other. It has a function which must be carried out with a different type of staff and methodology than the physical and design research which we have been discussing above. It is concerned with problems of organized complexity—with the prediction of the reaction of organisms (humans) in situations in which stimuli and reactions are interacting and exhibiting some degree of organization.

The objectives of a market analysis program are given by Heusner as the following: "(1) The investigation of the market for new products—including the testing of consumer reaction to these products, and (2) the investigation of marketing channels and the determination of the best distribution facilities. . . ." ⁷ Or as Nowland says:

The problem, therefore, becomes specifically—how can a manufacturer or product engineer ascertain the character and taste of the market for which he is designing or producing.

A hundred or more years ago, answers to such questions were easily obtained. . . . Today . . . the answer is to learn the needs of the buyer. Study, investigate, and evaluate the tastes, expectations, and requirements of the market, and, on the basis of knowledge so acquired, develop the product so as to fulfill the requirements defined.⁸

Such studies further include (1) proper styling to attract the market in the consumer's industries, (2) functional design and a knowledge of the requirements of other manufacturers in the capital equipment market, and (3) an understanding of the utility and quality requirements of products in all cases. It should be clear that satisfactory market research calls for creative ability in the fields of economics, psychology, physical sciences, and aesthetics. The proper organization, administration, and utilization of such research activity can play a most impor-

⁷ W. W. Heusner, "A Well-rounded Research Program: What Are the Elements in It?" *Sales Management*, July 1, 1945, p. 89.

⁸ R. L. Nowland, "Predesign Research as Applied to Product Development," *Mechanical Engineering*, Vol. 70 (March, 1948), p. 208.

tant role in ensuring that the research process in a given enterprise operates efficiently.

RAW MATERIAL AND WASTE UTILIZATION RESEARCH

Other areas of specific activity are those concerned with the raw materials entering into the manufacturing process and the utilization of waste materials from that process. Such activity may be included within product, process, or market research, and generally has for its objective the improvement of a cost situation. We have described earlier an instance where a better knowledge of the raw materials situation might have prevented a costly error on the part of one firm in developing and manufacturing an unnecessary substitute material.

The development of waste utilization techniques should not be overlooked in evaluating an enterprise's research program. A general survey of the waste products of all the manufacturing processes and a consideration of their possible economic utilization may indeed prove profitable. The experience of one large electronics manufacturing concern which found that the copper in the short lengths of insulated wire, which were cut off each time an assembly wire was cut to size, could be reclaimed at a considerable saving, is illustrative of this point. Waste disposal may be a social nuisance and require research for its abatement, as for example in the paper industry, where the utilization of spent sulfite liquors to make such products as commercial yeast is considered one answer to the problem of stream pollution.

ORGANIZATIONAL RESEARCH

An area in which profitable work may be carried out, but one which is not well known or recognized, is the field of organization itself. This area has been left largely to consultants in management, and only a few firms have seen the utility and profitability of continuing investigation into the matter of organizational relationships. The same considerations of methodology and personnel which we have discussed in this study apply to research in this category. As an example, the Standard Oil Company of California maintains a department on organization which carries out this type of work. Their efforts are

exemplified by several publications concerning organizational problems⁹ which are pioneering materials in their field. In the words of L. L. Purkey, manager of this department,

We have three principal functions:

1. Organizational planning;
2. Manpower control; and
3. Certain functions in connection with salary administration and standardization.

With respect to manpower control, we assist the departments . . . in establishing the essential workload, the number of people required to discharge the workload, their proper groupings from an organizational standpoint, and the design of the necessary systems to accomplish the work in the most efficient manner.

Taken together, the activities of the Department concerned with organization planning and manpower control include the undertaking of major reorganization studies which are carried out by members of the Department working on-the-ground with the particular segment of the Company under survey. . . .

The third principal function . . . relates to the Company's administration and standardization of wages and salaries. A section of the Department serves as a fact-finding group and statistical staff to those agencies in the Company who have the primary responsibility for the actual administration of the Company's salary plan. . . .

In the performance of these functions the work . . . is conducted on a thoroughly objective basis,—free from personal considerations which are regarded as most important responsibilities of Management.¹⁰

This example is cited as indicative of the fact that research as we have defined it can be applied to *any activity* of a given concern, if (1) its elements are understood, (2) its objectives integrated with over-all company characteristics and policies, and (3) the proper personnel and material resources provided so that the research may produce the profitable results that are desired.

⁹ Such as *The Co-ordination of Motive, Men and Money in Industrial Research*, by D. H. Voorhies (San Francisco: The Standard Oil Company of California, 1946), and *The Management Guide* by G. L. Hall (San Francisco: The Standard Oil Company of California, 1948).

¹⁰ L. L. Purkey, private communication, Mar. 3, 1948.

Requirements of Research Project Proposals

We turn now to the requirements of specific proposals to undertake work in any of the areas mentioned. The basic feature of such proposals is that they should be in accordance with the over-all plan for research activity in the enterprise. If they are not and if they appear desirable, then the general plan should be altered in such a way as to include the resources required for the proposed project. Thus, if the program includes only physical research and a proposal is submitted which includes market analyses, then it should either be rejected, or the requirements of this type of work be integrated within the company's organizational pattern. This is in harmony with our previous analysis of planning and method in which it was pointed out that, for efficiency, the problem should be analyzed and the resources which appear necessary to its solution be provided in advance.

The scope of these proposals should include *all the information that is available* on which a decision may be based, as well as the opinions of those concerned as to its merits. The need for the information should be obvious. The opinions of those who have an interest in the proposal are required to give a consensus as to the organizational attitude. It may also be repeated here that these proposals should be limited to those which are truly research, and that any which are made up of problems not requiring the resources of the research organization should be eliminated. On the other hand, *where problems exist and are recognized in any of the company's activities, there is no doubt that proposals should be submitted to management for research projects which will lead to their solution.* An active policy in this regard can, of itself, be extremely profitable in bringing to light inefficient and unprofitable conditions which exist throughout the company. Some simple standard means can easily be provided for the submission of such proposals. There is no danger in receiving too many proposals but in not recognizing problems or in approving the wrong projects.

The information which such a proposal should contain includes (1) *estimates of the personnel and material resources required for its solution*, (2) *the feasibility of attaining the solution*, and (3) *the results to be anticipated if the problem is solved.* It will

then be up to top management to accept or reject the project, in accordance with the basic criteria previously discussed. The information required may not necessarily be provided by the individual or department making the proposal. A number of different kinds of knowledge of the company's activities will usually be required if the informational requirements are to be met.

The estimates of resources required should include personnel, methods, equipment, as well as an appreciation of the channels of research which may yield useful information or results. These estimates will generally be provided by the research group which is to undertake the work, although in some cases information from operating and other technical departments may be required for their completion. Estimates of feasibility may be provided jointly by the research and operating departments involved. They should include the time which the project is apt to take, as well as some estimate of probable success. Where alternative methods seem to exist, they should be evaluated individually and estimates made of the possibility of shifting from one to the other, or doing both simultaneously. As we noted above, this part of the proposal is essentially exploratory research. An approach recommended for the chemical industry follows:

The entire group of alternatives must be abstracted. . . . That for individual processes should include remarks as to which assumptions used in the calculations are most critical, which may be the most doubtful; what the advantages and disadvantages of the process are with respect to health hazards, operating difficulties, development problems, ease of expansion; and, such factors as are not shown by the figures themselves. The overall abstract is usually a brief report recommending laboratory investigation of one of the proposals and showing why this one is selected in preference to the others.¹¹

It is also important to indicate whether the methods to be used are those of the laboratory, the plant, or the market. General considerations as to the correctness of the results anticipated and the probabilities of resolving all or part of the elements in the problem can be of great assistance. These data may be presented in the form of opinion alone, or in the form of opinion supported by empirical or previously obtained information, but

¹¹ "How to Increase Research Profits," *op. cit.*, p. 421.

the two should be sharply differentiated. In other words, where the estimates of resources and time required are completely intuitive, they should be denoted as such, and where there is some previous specific experience or experimental information on which the opinion is based, this information should be outlined. We shall see later that the continuing reports on the progress of projects in work should contain much the same type of information. It may appear that we are demanding too much of those who are charged with the task of preparing these proposals, and this at a stage when information is lacking, or vague or indefinite. *We can say only that industrial research is a matter of risk, and that the risks involved may be immeasurably reduced by the collection of as much pertinent information as possible concerning a proposed project, and a review of this information in the light of the over-all situation.* It is the same type of analysis which applies in the formulation of plans for specific actions by military staffs. The resources estimated as requisite to the attaining of proposed tactical objectives, time factors, anticipated results, the estimated enemy situation, etc., must all be considered from the standpoint of the over-all strategic situation, and a decision reached on a maximum of available information. That such information is difficult to obtain and that some of it is, at best, opinion, is granted, but no military operation would be undertaken without such an analysis.

The results which are anticipated if the proposed project is successful can be better estimated than the time and other requirements. The reason for this lies in the fact that the assumption *can* be made that these results are at hand, and an estimate established as to their effect upon the enterprise. Here caution should be exercised, since it is very easy to overestimate the benefits which will be obtained. Many a company has found the results looked forward to upon the successful completion of a research project were based upon mere exuberance and were not forthcoming. The possible patent situation resulting from successful work should be analyzed in this connection and given due weight in deciding upon a project. This presupposes at least a preliminary study of the patents involved, if they are not already well known in the enterprise. Fortuitous results cannot be anticipated, and no attempt should be made in that direction. If

the benefits are far beyond those expected, so much the better, but these extra dividends of research should not be counted on.

The preparation of these proposals probably should be initially the responsibility of the group which will do the work, even if it did not originate the ideas involved. Additional information which will be necessary should be supplied by the sales, production, and finance groups. In the large company, a specific committee covering all these departments may be designated to prepare research proposals to be submitted to management. In the smaller company, the analyses may be carried out by the research director in conjunction with a top management group. Where no research group exists, the technical staff should be represented, but if a full program is contemplated, it would be wise to add an outside consultant for additional information. In this connection, it may be pointed out that it is always well to consider the advisability of additional advice, which may represent considerable savings or profits if it is wisely chosen.

The analyzing group has a definite and serious responsibility with regard to these project proposals, and one that is often overlooked. It calls for the most careful and impartial analysis of each project, without regard to the effect top management's approval or disapproval will have on the research organization. This does not mean that the group should not present its opinion as to the feasibility of the proposal, but it does mean that the fact that a particular project would strongly affect the *size* of the group should have no bearing upon this opinion. It is not always necessary that the research department grow; in any event the desirability of having a larger group (or smaller, as the case may be) should not enter into the analysis of a given proposal. This is a matter to be decided after the entire program has been considered, and after those proposals which are desirable (in the opinion of top management) have been decided upon. *The research group should indulge in a great amount of self-analysis in this connection, with a view to ascertaining their functionality along those lines which top management feels will be most profitable.*

Summary

In summary, we have proposed certain criteria which should be applied to research programs and projects. These include

both strategic factors (concerning the financial aspects, areas of competition and markets, areas of possible research attack from the over-all standpoint) and tactical factors (regarding the feasibility of solution, estimates of resources and time required, and the type of research involved in the particular proposal or proposals). These comprise both the economic and the technical aspects of research activity. We have endeavored to point out that research should be considered an integral part of the whole operating structure of a company. *The understanding of this concept will lead invariably to the conclusion that research may profitably be applied to a multitude of problems in a business enterprise.* The consequences of the decision to undertake a research project give rise to questions of project magnitude which will be analyzed in the following chapter.

CHAPTER VII

THE MAGNITUDE OF RESEARCH PROJECTS AND PROGRAMS

Limits of Time and Money

When we have used the criteria discussed above in deciding upon the general desirability of a project or program proposed for research in industry, we must then answer specific questions as to magnitude. We may assume that a particular project has met the requisite strategic and tactical considerations of interest in a given company and has thereby obtained the approval of top management. It then becomes a matter of considerable importance, as far as industrial research efficiency is concerned, to consider means for establishing limits of time and money on the work to be undertaken in solving the problem under consideration.

From an economic standpoint, *the solution to a problem is worth some specific sum of money to an enterprise*. Although the solution may be valuable indefinitely, it is likely that *after a given time it will begin to lose its potential utility*. It is not implied, in the light of our previous analysis of the research process, that it is possible to establish absolute values for either time to be taken or money to be spent. We are here concerned with *limits*. For this reason, it will also be necessary to consider possible methods for evaluating a project once work upon it has been started. In other words, this entails evaluation of the progress of the research itself. When the factors influencing time and money limits and progress have been examined, we may approach, in conjunction with the basic operational elements already discussed, the design of an organization to accomplish our purposes.

Cost Elements

What are the cost elements in industrial research? They are represented by (1) the wages and salaries paid to personnel doing research or furnishing services to the organization and (2) the expenditures to supply and maintain buildings, equipment, and necessary services. Our present interest is in the influences of these factors upon the research process, and the intensity or rate of expenditures. The accounting for these costs will be considered in a later chapter. It has not been implied that all properly selected problems are susceptible of solution. However, it has been indicated, given the necessary combination of creative intelligences, methods of attack, and equipment, that the probability of success may be very high. The running cost of a project will be, of course, directly proportional to the amounts of these factors utilized during the time research effort is applied to the problems involved. Clearly, the cost of obtaining a solution will vary with the selection of the resources of personnel and material. Mediocre personnel applied at a less costly rate than more competent researchers will no doubt lead to more costly solutions, if they are obtained at all. However, we shall for the present purposes consider resources properly selected.

The rate of utilization of these factors and, therefore, the rate of expenditure will be influenced by the intensity of attack during a given period of time. By far the largest proportion of these costs is that devoted to the salaries and wages of all personnel engaged in this type of activity. In Table II McIlvain gives some general ranges for the percentage distribution of expenditures based upon a survey of representative organizations.¹

It has been estimated that the number of professionally trained personnel in research is approximately 50 per cent of the total personnel, and that *all technical personnel* amount to about 75 per cent, administrative, clerical, and maintenance making up the remainder. Since professionals are the most highly paid, it

¹ J. M. McIlvain, "The Research Budget," C. C. Furnas, ed., *Research in Industry* (New York: D. Van Nostrand Company, Inc., 1948), p. 149. Some 53 usable replies were received to questionnaires distributed in this survey, representing very small to fairly large organizations in 16 industries.

TABLE II. RESEARCH COSTS

| Item | Range, per cent | Per cent of samples in range |
|--------------------------|--------------------|------------------------------------|
| Labor..... | 60-77 | 69 |
| Purchases..... | 7-16 | 70 |
| Shop and craft work..... | 1- 8 | 78 |
| Utilities..... | 1- 3 | 64 |
| Depreciation..... | 1- 3 | 62 |
| Travel..... | 1- 3 | 74 |
| Miscellaneous..... | 2-14 | 80 |

is reasonable to conclude that, in general, at least 80 per cent of the labor cost is used to purchase creative intelligence and its technical assistance. This would then represent about 50 to 60 per cent of the expenditures involved in a given project. The logic of this proportion is clear when we recall the dominance of the personnel factor in solving problems. It should also be apparent that, except in unusual circumstances, the remaining costs are also largely determined by the number of professional and technical personnel involved in a particular project. This is due to the fact that the space occupied, the equipment manipulated, and the services utilized by a given number of personnel are restricted to some definite upper limit, which depends upon the nature of the problem.

Since the professionally trained researcher represents the largest single item in the average research budget, programs should be so organized and administered as to obtain the optimum use factor of this resource. *Equipment cannot substitute for the creative intelligence, but an adequate recognition of the fact that any replacement of a noncreative use of this intelligence by a less expensive factor can do much to enhance the efficiency of the process.*

Inefficient Use of Research Personnel

In a particular project carried out in a large research organization, the provision of proper equipment could have reduced the

cost by approximately one-third. In this case, exploratory work on a newly proposed mechanical process had been carried out with relatively simple apparatus, which had indicated its feasibility. It then became necessary to investigate the basic relationships, which involved a great many variables, and for this purpose some rather complex equipment was constructed. This equipment was not adequately planned for efficient use, as we shall see. After the work was begun, it was found that a minimum of four scientists and technicians were required to conduct the necessary experiments. In addition, it was extremely difficult to obtain the required data, and much duplication was involved. Inadequate replacements were provided for critical parts so that at least 60 per cent of the experimental time was devoted to cleanup, maintenance, reestablishing equilibrium, etc. As a consequence, the work was very costly and proceeded quite slowly. A decision was reached to redesign and rebuild the equipment with adequate instrumentation and replacements. It was then found that *one* scientist could conduct the experiments, with about 75 per cent of his time constructively employed in experimentation. Although this second piece of equipment was approximately twice as costly as the first, the savings resulting from its use were more than adequate to repay the added expenditure. If we assume that 100 days of experimentation might have been required in either case,² a simplified analysis of what the cost of this project might have been in the two circumstances (see Table III) is most instructive.

In this instance *one-half the time and two-thirds the expenditure* would have yielded equivalent results *if the necessary equipment had been adequately visualized before the project was begun*. Obviously all contingencies cannot be provided for in advance, but sound planning and adequate supplying of equipment can do much to improve the over-all efficiency of the research process. The above cost picture is not an adequate representation of this increase in efficiency, since it does no more than indicate the gain of 867 *professional man-days* to the concern. As we noted in Chapter IV, research efficiency is to be measured in terms of the correct solutions obtained, and certainly the application of the researchers' time (which could have been saved) to

² Actually the second piece of equipment was so constructed that less time was required to obtain the necessary data.

TABLE III. PROJECT COST COMPARISON

| Item | Equipment No. 1 | Equipment No. 2 |
|--------------------------------------|--------------------|--------------------|
| Initial cost..... | \$20,000 | \$40,000 |
| Personnel required..... | 4 | 1 |
| Personnel cost per day @ \$40/day * | \$160 | \$40 |
| Personnel (100 days)..... | \$16,000 | \$4,000 |
| Utilization factor..... | 0.40 | 0.75 |
| Total personnel cost..... | \$40,000 | \$5,333 |
| Total working time, required days... | 250 | 133 |
| Total cost of project..... | \$60,000 | \$45,333 |

* Including overhead for services, etc.

other problems would have greatly increased the possibility of obtaining answers to these.

In a sense, all the elements of cost mentioned fall into the category of reinforcing the ability of the creative personnel to apply their talents efficiently to research problems. It is very possible that a considerable increase in the utilization factor of scientific personnel can be accomplished by means of additional expenditures in these categories with only a minor effect upon the over-all cost. As Voorhies noted in the Standard Oil of California survey mentioned previously, "A complete study of these aspects in one of the participating companies leads, however, to the conclusion that overall economy is on the side of providing the more expensively manned research organization with readily available services within their own organization."

Cost as a Function of Time

The rate of intensity at which it is decided to undertake a given project has, of course, a direct effect on the rate of expenditure and may also influence the total cost. In many cases, it may be said that, for equal personnel efficiency and ability, the more concentrated the attack upon a problem, the more costly the solution. Admittedly, this may not necessarily reflect upon research efficiency, since the provision of the correct solution at

a time when it is required may be what is desirable in a given instance. The point is that, although we may analyze a project, resolve it into its component problems, and synthesize a group or groups equipped to solve these problems, these problems and their solutions may not be susceptible to efficient attack by more than a limited number of personnel. Additional personnel can only attack these same problems, and while the chances of success may be improved, the time required may be the same in either case. It is only when the resources applied are increased disproportionately (as in the case of the wartime atomic energy development) that time is certain to be saved, and this at the expense of greatly increased costs.

Irrespective of the intensity of the research attack, the phase of activity into which a given project falls will influence the expenditures required for its completion. This is dependent largely upon the conditions surrounding the problem. Thus, if neither the method of solution nor the data required to obtain the desired predictive results are sufficiently known, the approach must necessarily be exploratory. In a sense, exploratory research connotes extensive activity, since even a concentrated exploratory program involving only one problem must proceed by virtue of the different lines of attack pursued by individual researchers or scientists. We have chosen, therefore, to designate the next stage as intensive. Here, either the method of solution or the data necessary will have been *indicated* by the exploratory work, and the research will be undertaken with the specific objective of obtaining these. Beyond this second stage, problems involved in translating these results into usable industrial terms must be solved before the research is successfully completed. It is here that model and user tests, pilot plants, and in the case of market research, restricted-area tests, and other devices are utilized to ensure that the probability of error in predictions based upon the research results is reduced.

Generally, the cost of exploratory research is less than that of intensive research, and this, in turn, less than that of the transitional stage. It is also true that the proportionate costs listed previously will vary, depending on the phase, equipment, and services, often being larger in the last stage than in the other two. This is not a hard and fast rule, since it is possible that quite expensive apparatus, such as electron microscopes,

molecular stills, electronic computers, and cyclotrons, might be required in the exploratory phases.

In any event, it is generally agreed that exploratory research may be undertaken for the least cost, although the general probability of success in this stage may be lower than that of succeeding work which is properly organized and administered; *i.e.*, as

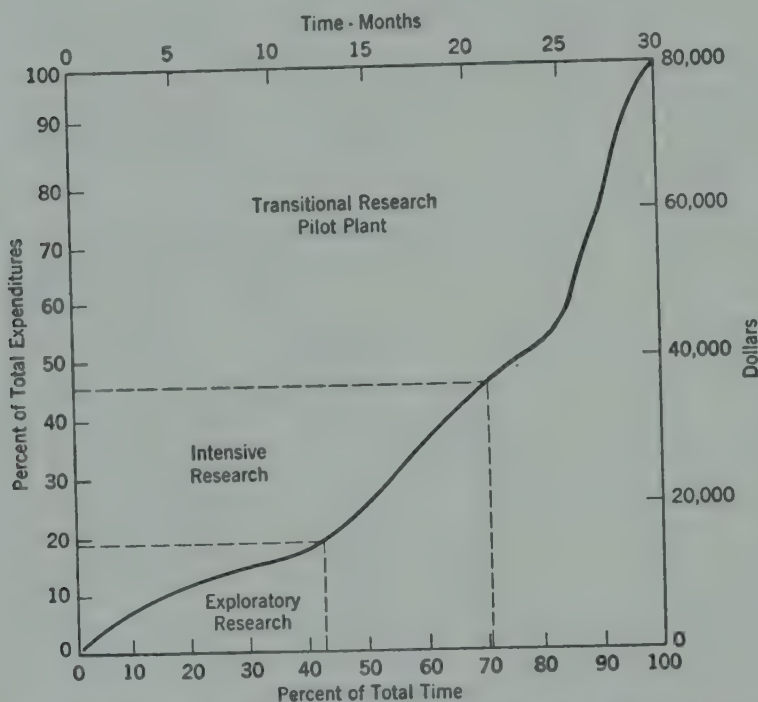


FIG. 6. Relationship of expenditures and time for a complete research project.

the project approaches completion, the total costs and quite possibly the rate of expenditure will increase, but at the same time the probability of arriving at a successful solution should also increase. An analysis of an actual project in these terms is shown in Figure 6. This project, which involved a new chemical process, cost approximately \$80,000 and required 30 months from its inception to the turning over of the necessary data and specifications to the engineering department of the particular company for the design of a plant. If percentage expenditures are plotted against percentage of time expended, each being determined from its respective total, the three distinct phases of the project are quite distinguishable. In the exploratory stage some 19 per cent

of the total cost was expended in 42 per cent of the time, at a rate of approximately \$1,100 per month. When it was decided that two of the methods developed by this work appeared promising, the intensive phase was begun to obtain further data and solve certain problems so that the process might be piloted. This stage required an additional 26 per cent of the total expenditures which were used in 28 per cent of the time, at a rate of about \$2,600 per month. At the end of 21 months it was felt that the project warranted the construction of pilot facilities for the solution of the remaining problems and to enable it to be translated to production. This last phase required 55 per cent of the expenditures at the rate of approximately \$4,900 per month for 9 months, or 30 per cent of the total time.

This example is indicative only but is interesting in this form since it gives us a means for analyzing and comparing research projects. The points of transition from one phase to the next are often not so clear-cut as in this instance, and in some cases exploratory and intensive research work may be minimal or not required at all. This is true in much product development work. The general trends indicated by such information are of importance in considering the scale on which the research involved in a particular project is to be undertaken.

How Much Is a Research Project Worth?

An understanding of the relevance of the various cost and phase factors is necessary in arriving at an answer to the question of what scale is desirable. However, this can serve only as a background in making the basic decision as to the amount which should be expended. If a proper analysis of a proposed project has been made, in general accord with the criteria outlined in the last chapter, the results to be anticipated if the work is successful will be clearly outlined. Basing its decision upon these data, management must decide what the project is worth in financial terms. At first glance this would appear to be a very difficult question to answer, and indeed in many enterprises no attempt is made to answer it. It should be recalled that, as always, we are interested in limiting conditions, and not in the exact amount which is to be either appropriated or spent. If we can set this upper limit for a particular project, then we have a

portion of the answer to the question of magnitude for the given case. This limit is similar to that noted in connection with the size of the over-all research program, and the answer may be formulated in much the same terms.

For example, the first criterion is the *necessity* of obtaining the answers to the problems involved in the project. It should be possible to state that the lack of these answers will be more or less costly to the enterprise, or that the achieving of the objective of the proposed project will enable the concern to carry out definite plans with regard to increased sales or lowered costs. On the other hand, while the solutions may not be immediately required, it may be desirable to have these answers in reserve to meet some anticipated or unknown future contingency. If none of these circumstances is the case, then the project is worth nothing and need be given no further consideration. Hunches and gambles on the part of management have been known to pay handsome returns, but for every one that has, many more have been complete failures. Rational research planning has no place for the illogical or unfounded guess.

If a project is necessary, then its solution has some value to the enterprise. It is this value which management must specify in order to designate the general order of magnitude for undertaking the work. This appraisal may be made with the least difficulty if an objective attitude is maintained. The problem can be approached from the standpoint that the completed results, or those anticipated in the analysis of the project, are being offered, and that a price must be bid for them. This price will then be influenced by (1) what the project is worth and (2) what the enterprise can afford. If the firm could afford to buy the completed results for a specified sum, it could also afford to expend this sum in research on this project,³ *if the results were guaranteed.*

Since research usually cannot make such guarantees in a specific instance, the amount will represent only a limiting or bounding sum, and the feasibility or probable success must be taken into account in making a specific appropriation. This may be accomplished by first setting the over-all limit to determine relative magnitudes as between projects, and then establishing

³ Provided the timing is satisfactory. This is discussed below.

the same type of limit for the specific phase involved for the particular one under consideration. Since successful projects must carry the burden of the unsuccessful ones, a factor of safety or margin must be provided for. *The same factor should not be applied to every project, since questions of technical feasibility, desirability, or necessity will have an important bearing in each instance.* This factor or margin for the entire program should be such that a reasonable degree of safety exists.

It will be recalled that both the cost and probable success of a project will often increase as the work moves from exploratory to intensive to transitional phases. Thus, an adequate limit may be set for exploratory work in this manner—*what would be paid for the results of such a study*—keeping in mind that these results would be only indicative of the best avenues of intensive research. The same analysis can be used throughout the life of a project. It is apparent that by following this method, of first setting the over-all value and then the value of each phase, the enterprise may be assured at least that expenditures on a given project will not exceed the value of the results anticipated, in the best judgment of the firm's management.

AN EXAMPLE OF PROJECT VALUE ANALYSIS

As an example, let us consider the project analyzed in Figure 6. The initial value which management might have placed on the project perhaps was \$250,000 to \$300,000, *which is what they would have paid at that time for the successful results.* The analysis to arrive at this sum would have included, of course, the possibilities of capital recovery and the present worth of the results. At the same time, it would have been possible for management to set a value on the results of an exploratory investigation, say \$25,000. Now, the over-all magnitude of the project has been established, and an estimate of the cost of the preliminary investigation from the research department (for, say, \$10,000) can advisedly be approved.

When the preliminary work has been completed, at a cost of \$15,200 (as we have seen), it is proper to review once again the over-all value of the project to the enterprise, which may not necessarily be the same as the first estimate. Then a value for intensive research may be established, *if it is decided to continue*

with the problem. In this case, this sum might have been \$50,000, and an appropriation approved for \$25,000. At the conclusion of the intensive laboratory work, the same type of analysis would be utilized to arrive at a value for the pilot plant phase. Thus, based on these figures, when the project is completed successfully at a total cost of \$80,000, there is no doubt as to its worth to the concern, and that the research was profitable. Operating in this manner, it is possible that the potential failure to arrive at a successful solution would have been indicated at one of the intermediate stages. If this were not the case and if the project were unsuccessful after the pilot plant operation, then the increment of potential gain over actual cost for the entire research program must be depended upon to make up for the losses. There are also intangible results which are of value, even in unsuccessful projects, and these should not be overlooked. The very fact that the project was undertaken in a scientific manner and that data are available for future use may be of great assistance in evaluating and solving other problems.

This type of analysis, if continuously and objectively applied to all research work in an enterprise, will enable the economic factors to be rationally arrived at and make for adequate control of the process. It should be understood that we have not implied that management in setting these values and the others to be considered is directing the process. This direction should and must be accomplished by the research administrators. Management, in determining what desirable and worth-while (and technically feasible in the eyes of the research administrators) work should be done, is merely carrying out its proper function in the enterprise. It has the same responsibility to set this type of policy for the production, sales, and other divisions. Despite the active proponents of the principle that projects shall be chosen by research administrators alone, it is generally not efficient to do so.

In times of increasing production, it has been common practice to leave the choice of projects in the hands of these administrators. On the other hand, it is to the advantage of research personnel to accomplish work which management considers of greatest value. This is particularly true when sales and profits are declining. The former attitude is probably advocated as a defensive mechanism by those in research whose

efforts have been hindered and perhaps seriously affected by external direction. Management has a serious responsibility in this regard—to choose wisely from the proposals submitted to it, and to indicate the value of these proposals to the enterprise, but not to attempt to influence the manner of their prosecution. As Bichowsky says,

Invention, thus, is not an esoteric art, practiced by a genius in some attic on somebody's cookstove—at least, not usually. Under modern conditions, it is a process of production, which is just about as controllable as any other production process of this machine age.⁴

The Time Factor in Research

Although we have considered the monetary scale on which a project may be undertaken as a distinct phase in its evaluation and appraisal, in reality it cannot be separated from the temporal factors. Time is an integral part of cost, and it is only in the interest of clarity and convenience that we have considered it separately. Godwin makes an interesting analysis of the relationship of time to research activity:

To begin with, research takes time for fruition. Lots of it, as a rule. Things have to be done in logical sequence, and frequently the second step is not apparent until the results of the first are in. It's not a simple question of man hours. Up to a certain point the job can be speeded by increasing manpower; but beyond that point, if you expect to complete a research project in half the time merely by doubling the staff, then you might just as well try to cook a three-minute egg in one and one-half minutes by using two cooks. Furthermore, the total elapsed time for solving a problem depends upon the problem and bears absolutely no relationship to the personal wishes of the businessman, his board of directors, or his sales and advertising departments. . . .

The player who withdraws from the game before the hand is played out never wins the pot. We have seen examples of one-year research projects that produced nothing tangible in eleven months and then hit the jackpot in the final weeks. Much depends upon the organized plan of attack, which may be such that a final integration of facts cannot come until the last stage. The fact that each of the research-

⁴ F. R. Bichowsky, *Industrial Research* (Brooklyn: Chemical Publishing Company, Inc., 1942), p. 46.

er's monthly reports does not contain a startling new discovery does not mean that progress has stopped.

There is need for patience in research. Otherwise let's not undertake it.⁵

Despite the fact that time cannot be absolutely controlled in research, as we have freely granted herein, *it is necessary to establish the limits within which the resolution of the particular problems will be of value to the enterprise.* In this respect, industrial research differs from much institutional research—the absence, or at least the nondominance of the economic factor in institutional activity usually removes from it the burden of having to meet changing market conditions with the solutions to its problems. Once again, there are no hard and fast rules, but an objective rational approach is apt to be more efficient than the subjective guess.

Time is required in industrial research to (1) assemble data, study previous work of a similar nature, define the problem, and arrive at hypotheses as to possible avenues of solution; (2) decide upon, design, and assemble apparatus and methods of attack; (3) conduct experiments to collect the desired information; (4) arrive at a creative solution with regard to the relations between the variables under consideration; and (5) verify the solution and extend or extrapolate it to the required full-scale conditions. Each of these elements involves its own temporal factor, as we have seen. In a very general sense, this time is equivalent to money, or as Barnebey says,

Time and money, like matter and energy, are interchangeable under certain conditions, or convertible one into the other. If an established or potential market for a new material exists, a production plant in operation today is worth a certain amount of money. A year from now it is probably worth somewhat less. In ten years it may be almost valueless.⁶

In establishing a temporal scale for a particular project, general means for reducing the time of the various elements should be considered. The provision or use of well-organized services

⁵ F. Godwin, *To Our Sponsors*, 2d ed. (Chicago: Armour Research Foundation, 1945), p. 17.

⁶ H. L. Barnebey, "Time Equals Money," *Chemical Engineering*, Vol. 55 (June, 1948), p. 103.

to provide the literature, patent, and other reference data in the exploratory stage can save much valuable time which would otherwise be needed by the worker. The postulation of possible methods of attack and the decision as to experimental apparatus are creative activities, and the time required cannot easily be predicted. However, the researcher can be assisted by the provision and use of properly manned and administered project groups. As we have indicated, creative results are most apt to be produced by the scientific team possessing a sufficiently comprehensive background so that all the problem subelements may be understood and resolved.

The reduction of the time required to conduct experiments is contingent in large measure upon the design and planning of the apparatus and the data which are required. An example of the savings in time which are realizable through the use of well-designed equipment was described earlier in this chapter. The understanding of the need for adequate methods of obtaining significant data is of great importance at this stage. As Bowden points out, in describing methods of industrial experimentation,

It was evident that to apply tests of significance conveniently and economically the experiments had to be planned in appropriate forms. It is considered that the methods outlined should be as much a standard tool of the industrial experimenter as a chemical balance is of the laboratory experimenter.

It should be apparent that, if the apparatus and method will not yield significant results, the derivation of significant relationships from any data collected can only be a matter of chance. *Reliance upon the possibility of random success in experimentation is not likely to minimize the time of the process.* Bowden continues,

In carrying out an industrial experiment the choice is not between using a statistical design with the application of the appropriate test of significance or the ordinary methods: the choice is between correct or incorrect methods. Even the simplest experiment requires an estimate of the significance of its results.⁷

The verification and extension of the results of intensive laboratory work often can be done very quickly if the necessary

⁷ R. C. Bowden, "Foreword" to K. A. Brownley, *Industrial Experimentation* (Brooklyn: Chemical Publishing Company, 1947), p. 3.

tools, such as pilot plants and field survey organizations for market analyses, are at hand. The time required at this stage generally will be directly dependent upon the material resources furnished to the organization. We have already seen that this is apt to be the most costly phase of the work, and also the phase with the highest probability of success.

SETTING A LIMIT ON TIME

The determining of the upper limit for the time factor of a project is similar to that described for monetary expenditures. The questions to be answered are (1) *How long can the anticipated results be profitably awaited?* and (2) *How much time can be allowed to arrive at the results of each phase?* The second question is clearly affected by the answer to the first, which is within the area of responsibility of management. The over-all time limit for the project can be reached in terms of the cost of not having the results in a specified time. The judgment of management, considering sales, production, finances, etc., must come to a conclusion as to how long the enterprise can *afford* to wait for the anticipated results. If the value of these results will not diminish within a long period of time, then the temporal scale of the project may be similarly extended. This is not intended to imply that this should be the time allotted to the project—from the research organization's standpoint this may be neither necessary nor desirable. For example, the research department may have idle man power available at a specific time, and work on a long-range project may be intensified in order to utilize the resources available to best advantage.

It also may not be possible to meet the desirable time limits, or the monetary limits established in a similar manner, because of technical considerations, but the setting of these limits will tend to ensure that the work done will be usable and will not cost more than it is worth. If the results to be attained become rapidly less useful to the concern, then certainly the over-all time allowance must be set at a correspondingly low value. Based on this type of decision, projects may be designated as to their relative temporal magnitude, such as short, medium, and long range. The absolute values of these designations will vary depending on the industry and the nature of the research work

done. In some industries, where many projects are completed in a matter of months, a project taking as little as a year would be considered long range. In other cases, one year may be short range, and five or ten years long range. It will be also possible to establish priorities for the research work, based on the monetary and temporal values for approved projects determined by management.

This project classification must then be utilized by the research organization in planning the work to be done in connection with a given project. The decision as to time which can be allotted to the various phases of a project is much more a technical research matter than the determination of over-all temporal value. It is probably not desirable or possible for management to attempt to set these particular limits since they are drastically affected by the latter value. It would not usually be proper to spread a low-cost low-return project over a number of years since it is possible that, when such a project is finished, the need might have disappeared. It would also be improper to attempt to compress a truly long-range project into a short-time interval, since this, too, might lead to inadequate or unsuccessful results.

The specific criteria to be considered when setting the time limits for a project are (1) the ability of the enterprise to take advantage of the results, (2) the effect of time upon the correctness of the anticipated results, and (3) the effect of time upon the value of usable and correct results. The last point has already been discussed in connection with the consideration of the cost of not having the research work done. It is particularly important with respect to new products and competitive factors. Research work leading to the evolution of a new product whose anticipated market has disappeared with the passage of time between the project's inception and its fruition is of little, if any, value. Similarly, research leading to lower production costs is fruitless if competition has, in the meantime, captured the market with new products or still lower prices.

The effect of time on the correctness of the results of research is of particular importance in problems in which factors of organized complexity are involved. A market analysis spread out over a number of years may be of little value when the results become available, since the economic factors upon which

the work was based may have completely changed during the time. Such considerations would not ordinarily affect problems of simple variables or disorganized complexity, since changes in conditions or in the knowledge used to attack the problems should be readily available during the course of the work.

As far as the ability of an enterprise to take advantage of the results of research is concerned, the time factor may be affected in a number of ways. As has been indicated, the results of research are likely to require capital investment of one kind or another. If the concern is in the midst of an expansion program or is handicapped in obtaining capital for other reasons, then there is no rationality in intensively pursuing a new product development which will require extensive plant facilities. If it appears that the long-range situation may be more favorable, then the project should be allowed to mature slowly. The fact that the market, or other elements in the economic or production pattern, may not be ready to assimilate a new development is also a significant aspect of time factors.

The history of television research is illustrative of the importance of the interacting elements of an over-all problem. In the research which led to the present status of that industry, it was necessary that individual projects be coordinated from a time standpoint, since the ultimate market depended upon all of them. An enterprise which developed an excellent receiver before satisfactory transmission devices, not to mention networks, were available, was in no position to capitalize on its work, and in so far as monetary returns to that firm were concerned, the research was of little value. Pickup devices, reproduction devices, standards of performance and operation, over-all field testing were only a few of the factors which affected the end result. We see, therefore, that time allowances and limits in industrial research cannot be established arbitrarily. For efficient research, the temporal scale must be integrated with the economic and scientific data relevant to the particular project.

The requirement that the time and the economic factors be ripe in order that results may be profitably utilized is as true in industrial work as in the accumulation of new scientific knowledge. The recent development in the United States of a new process for organic chemical synthesis is illustrative of this

fact. The history of this process, as described by Riddle, points this out quite clearly:

A minor experiment conducted by chemists at the Bureau of Mines in 1930 has, in recent years, borne unexpected fruit. The chemists were investigating the Fischer-Tropsch synthesis—then a new and not fully understood process for making synthetic gasoline from carbon monoxide and hydrogen. In the course of an experiment to determine just what went on in this catalytic reaction, ethylene was added to carbon monoxide and hydrogen and the three were passed over the process catalyst. A complex product was obtained that consisted more of oxygen-bearing compounds than the hydrocarbons sought in synthetic gasoline, and the study was carried no further. . . .

It has now reappeared in this country as one of those rare achievements in chemical synthesis—a new method of making organic chemicals. . . .

The development . . . was done in Germany where short supplies of many raw materials for chemical manufacture have been a continuing incentive to research in chemical synthesis. Some German research chemists saw in the report the intriguing possibility of making chemicals from such raw materials as ethylene, carbon monoxide and hydrogen. Research indicated that the reaction was a general one for nearly all ethylene-type compounds. . . .

Eventually, a patent was issued in America covering some aspects of the process, but the American chemical industry showed only mild interest. . . . When the war was over, however, and our investigators returned from Germany with detailed reports of how the process could be used for manufacturing alcohols, the possibilities of the new method of synthesis became apparent. . . .

Products from the . . . process appear to have a promising future . . . based on readily available, relatively inexpensive raw materials; . . . synthesized by straightforward, commercially feasible processes.⁸

Evaluating the Progress of Research

After the temporal and financial magnitude of a project has been determined, and the work of the research organization has begun, it is necessary to have some means of evaluating progress. It is not sufficient for management to appropriate the estimated funds and indicate the most desirable time of completion—the

⁸ E. H. Riddle, "The Oxo Process," *The Rohm & Haas Reporter*, Vol. 7 (February, 1949), pp. 21ff.

work must be evaluated (and continuously reevaluated) in the light of results obtained and changing economic and scientific factors. Specific progress upon a project may be defined as the obtaining of partial, complete, or negative answers to the various subproblems involved. The creation and recognition of new problems fall within this category, as does obtaining data or methods with which to proceed further. In a sense, each phase of a project represents a major group of subproblems, and it is the recognition of the solutions obtained within these groups that constitutes progress evaluation. It is not merely the desirability of suspending work when negative results are reported—actually this may not be the case at all. The comprehension of the results being obtained can lead to a reevaluation of the project in terms of all the aspects we have been discussing; the changing of its priority, the allocation of additional monetary support, more or less time, etc. In other words, sound planning calls for an analysis of the work in progress as well as that proposed. As Magos has written in this connection:

The strongest reason for discontinuing any project is to substitute in its place one which appears to be much more promising.

The evaluation of a research project should precede its inception and should continue at frequent intervals long after its completion.

The most we can expect is to eliminate projects of poor promise and accelerate a few of the most hopeful ones.⁹

This evaluation must be undertaken on the basis of reports from the research organization. These reports should not be full of glowing stories of progress which will be made, of answers which will be obtained, or of excuses for the status of the work. On the other hand, management in evaluating the results reported should not expect that "each of the researcher's monthly reports . . . contain a startling discovery," or feel that otherwise "progress has stopped." The reports should contain information on the questions which have been answered to a given date. If a number of subproblems are involved, then the reports to management should be a consolidation of the information available to the research administrator in charge of the entire project. The information should be in sufficiently nontechnical

⁹ J. P. Magos, "Evaluation of Research Projects," *The Frontier*, Vol. 10 (December, 1947), p. 2.

terms so that management can answer the question: *Can we, or should we, take any action upon the information which has been supplied?* The time taken and the funds expended should be considered in the light of what remains to be done and the limits which have previously been established. If, in each of these reports, a reconsideration of the opinion of the research organization as to the anticipated costs and time is given, a metrical evaluation of progress will be possible.

CHARTING RESEARCH PROGRESS

A chart similar to that in Figure 6 may be used, on which time and money consumed are plotted as percentages of the estimated totals. The use of such a measurement should not imply an overrigid control of research activity, but can ensure that the work will follow a pattern comprehensible and desirable to management. Such a chart is shown in Figure 7, which is the same project plotted previously in Figure 6. The plotting of four successive reports, at 6-month intervals, is shown. Progress may be followed from period to period, and it should be apparent that the degree of recession of a given point toward the origin, as re-estimates are made, is a measure of actual progress toward having the solution in hand. For example, the \$10,000 spent in 6 months as shown in Report A, shows up as 25 and 30 per cent, respectively, of the estimated time and money to complete at that stage (point 1A). This progressively recedes with decreasing increments to 12 and 20 per cent (point 1D) in Report D. When this point remains relatively unchanged in successive reports, it should be clear that, *in the opinion of the research department*, the project is nearing an end. Refinements of such a method could be described, but since the use of such a device to measure and record research activity is not yet common in industry, they are not warranted. It is felt that a forward-reaching step in the understanding of the nature of this type of industrial activity would be taken if these measures were utilized. If the results of a great many projects were available in this format, more precise formulations would be in order. Such charts of various types of projects could be published in percentage form, without revealing any confidential data.

The objection may be raised that such charts are uninforma-

tive compared to detailed reports of progress, or that the information contained may be readily available to those interested in research activity in other forms, or perhaps that it already

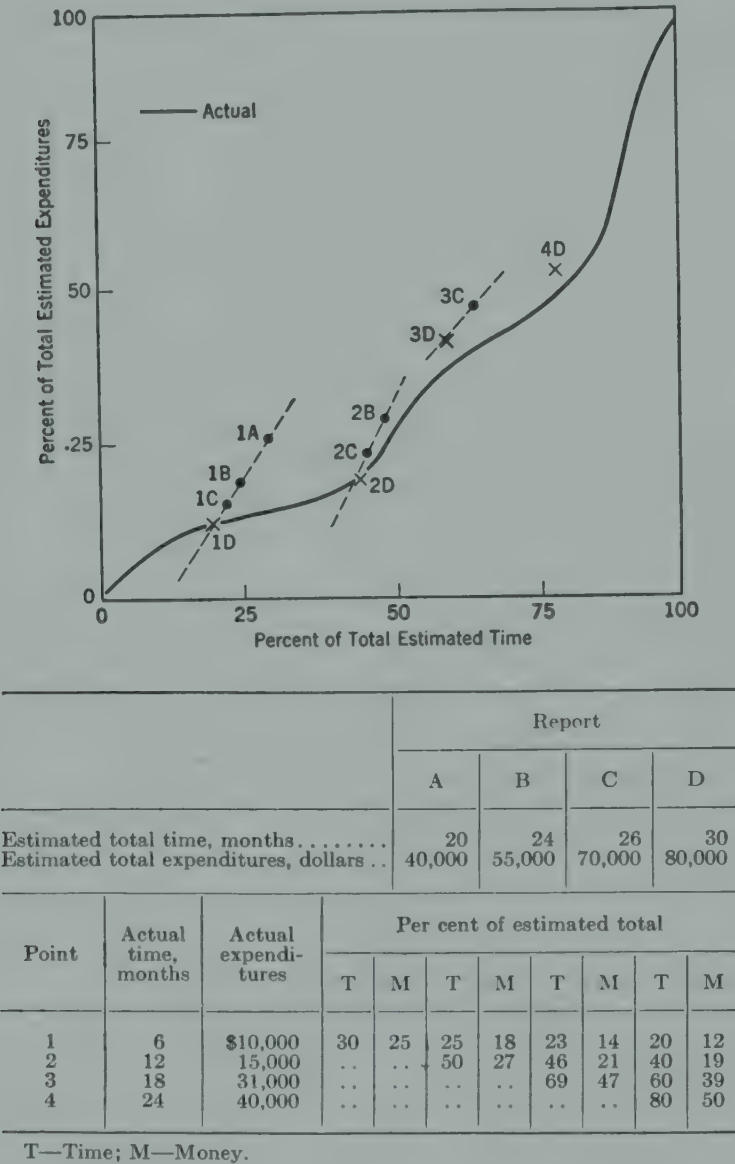


FIG. 7. Progress chart of a research project.

should be known to all those concerned. Admittedly, in common with all forms of communication, the information which may be presented by such charts is limited. However, it should be remembered that these are intended for management's evaluation and, since the estimates of time and funds which will be required

should be made by research personnel, represent research's own story of its progress. Information as to past history and future prospects on a great many projects can be presented for quick and ready appraisal. A summary chart can present the same information for the entire program. Where reevaluation appears to be required, then detailed analyses may be made. *Research administrators should have no hesitancy in presenting management with information of this type. They should feel that they have the same responsibilities in this regard for their own very special and very different field, as, for example, do production managers.* Management, for its part, should assist research in producing desirable results by objectively scrutinizing such data with care to make certain that the activity indicated is in accordance with desirable company policy *but without attempting to engage in the actual direction of the research.* If the research direction is not in accord with its wishes, then the administrator should change it or should be replaced, but management, through research committees or otherwise, should not attempt the task of directing the actual work. As Holland has said,

Management expects the research organization to operate as a "human engineering" laboratory, in which the man at the laboratory bench, the trained scientific worker, equipped with the proper tools of his profession, is the real motivating spirit of the research organization.¹⁰

The reevaluation of a particular project in the light of funds and time to be expended may be undertaken for a number of reasons. The fact that progress is slow (successive plots of the same point on the chart mentioned would be receding rapidly toward the origin) is perhaps the least important factor in making any changes. However, where such is the case, it is the responsibility of the research administrator, not to explain the reasons for the lack of progress, but *to present a clear, concise, and objective picture of the feasibility of further progress.* In view of such analyses and with the estimates of cost and time at hand, as well as the previous reports which should have been made, management can decide on the present value of the going project to the enterprise. If the groundwork, which we have indicated as necessary, has been properly laid, the data on which

¹⁰ Cf. *Proceedings, Illinois Conference on Industrial Research*, address by M. Holland (Chicago: Armour Research Foundation, 1948), pp. 19f.

to base this decision are readily available. This decision will also be a matter of judgment and is a responsibility of sound management. *There is no need for hasty decisions to remove a project from the agenda, nor is there any virtue in concluding that once a proposal has been approved, the work should be carried on as long as a researcher desires to continue it.* To protect themselves from the first of these unsound policies, many research administrators have insisted upon the second. In the long run neither can benefit the enterprise nor the research organization.

A more cogent reason for reevaluation is the *type* of results which are being obtained by the work. These may indicate that further work is not technically feasible by the given organization or concern, or that more promising avenues have been opened up which would indicate the desirability of changing objectives and perhaps setting up new projects. Or, the data obtained may be extremely promising and point the way to more useful and profitable results than originally visualized. This might call for additional funds and more intensive activity.

Changed competitive situations, financial factors, and other considerations will also have a direct bearing on the value of a project during its life. As reports are made to management, such determinants may necessitate changes in the magnitude of a project or program. *Whatever the reason for placing new values on the objectives of the work in progress, the judgments should be based on the most pertinent information available at the time.* We see that the criteria for initial approval, the establishment of values for monetary and time limits, the evaluation of progress, and the reevaluation of projects and programs are all part of a continuing objective analysis based upon both scientific and economic factors. Only this type of analytic approach can ensure the optimum research efficiency in industry.

Translation of Results to Productive Use

Once a problem has been resolved to the satisfaction of the researchers, the transition to production, or to full-scale operation, or to company-wide utilization, as in the case of market researches, is necessary. This is not an easy task and is an integral part of any truly efficient organization. A number of prob-

lems of quite a different nature from those that we have been discussing arise here, and as Patterson says,

It is here that the success of industrial research effort often falls down. We shall do well to enumerate some of the causes of this breakdown:

1. The intrinsic difficulty of proving sufficiently definitely the practical soundness of our deductions amidst all the variables that can vitiate trials.
2. The fact that at this stage we have to satisfy and often work through the production personnel in the factory in our effort to show that we have achieved something useful.
3. The serious expense usually involved in the process no matter whether we are applying our results to the production plant itself or in a try-out unit.
4. The reluctance of those responsible for the production plant to allow it to be interfered with, or, in any case, to tolerate the interference of amateurs in subjects they feel they know so much more about.
5. Not infrequently the trouble has its origin in two or three causes operating simultaneously.¹¹

The point is that, with the experimental solution of the problem by the researcher, further work begins to extend into areas beyond his control. An increasing number of personnel begins to be involved in translating his results into profitable activity. The costs of the work begin to increase, and although the risks involved in obtaining answers to the research problems themselves decrease, "the real business risk starts." Manning continues in this vein, saying, "At this stage opportunities can be lost easily or business enterprises made. This is always more costly of time, effort and money than the research itself."¹²

This transition to productive use must be made with careful planning and coordination of all the activities which may be involved—production, sales, personnel, traffic, finance, legal, and other departments may have a part in making the results useful. A final evaluation of the research results in conjunction with such of these as may be called upon to cooperate in this last stage is most desirable. *If the results are sound and have a high*

¹¹ C. C. Patterson, "Conversion of the Results of Research into Production," *Mechanical World*, Mar. 29, 1946, p. 351.

¹² P. D. V. Manning, "Putting Research Data to Work," *Chemical Industries*, December, 1948.

probability of being correct, it is at this point that the research organization will be called upon to "sell" its work. The necessity for mutual confidence and understanding as to what the objectives are cannot be overstressed. A stepwise procedure on the part of the research group in gradually extrapolating its results to larger areas can be of great assistance in creating this confidence. The mistake is often made of proceeding to full-scale activity on the basis of insufficient data. This can be true of all the types of research that we have described, and the value of a most searching and critical self-analysis of the results should not be overlooked. One additional transitional phase can often mean the saving of enormous expenditures. Nevertheless, there must come the time when these results will begin to repay the enterprise for the funds invested in them. The decision as to this time must rest with the research organization, and lengthy postponements to clear up noncritical or minor factors can be of little benefit.

As an example of the difficulty involved in the transitional stage, we may cite the experience of one large research organization in which a too rapid transition proved very costly. The work on a new process had proceeded to the point where a laboratory model was operating very satisfactorily. Not all problems had been solved, but it was hoped that a pilot-sized unit might answer the remainder of the questions. However, owing to the desire of management to put the process into production, it was agreed that a semiworks unit should be set up. Owing to its size, some apparently minor design changes were required from the equipment as operated in the laboratory. These were recognized, but it was felt that certain features of improved design would overcome any of the unknown handicaps which might appear. The unit was expensive and took a long time to build. Upon its completion, it was found that it would duplicate *most* of the results of the laboratory unit, but that it would not produce the particular quality of material which management had been so anxious to obtain. This was ascribed to the minor changes. Since changes in the semiworks unit were difficult and costly, the project was eventually abandoned. An intermediate unit would have obtained the same results and, perhaps, the answers to the problems.

On the other hand, there have been numerous instances where

projects which should have been translated to production were kept running in laboratories and pilot plants with continual changes and little improvement in results. Some of these could quite easily have overcome their difficulties by increase in size. Others should have been abandoned. As is the case with all research, there is no a priori rule—the combination of sound scientific with sound managerial judgment cannot yet be replaced by definite formulas. Each instance is a case unto itself and must be treated as such.

It is possible to obtain the data on which full-scale results can be predicated in a number of ways. Laboratory replicas of proposed production units may be one step, but unless the problem is clearly one in which the scale factors are negligible, this is probably not sufficient. The pilot plant, or the restricted-area trial, is a very satisfactory phase in this transition, providing the emphasis is placed on production problems. Of course, the pilot plant may be a very vital tool in the solution of research problems in their earlier stages, but this is not under consideration here. In some cases, production from pilot plants or pilot assembly lines may be sold and thus serve to reduce the costs and also to provide advance market data. User trials of new equipment are also very worth while. Experiments may be made with the actual production plant, and if this is done, great care should be taken to provide adequate controls and collection of data, so that the results will be not only significant but also meaningful to production personnel. If a new product is under consideration, then a great many types of engineering, market, production, and quality evaluations will be in order during this transitional stage.

When all these factors are taken into consideration and projects have been carefully examined, appraised, and planned from their proposal to their final utilization as a part of the enterprise's profitable activities, the risks of industrial research will have been correspondingly reduced. These methods will then have served partly to achieve our objective of increasing research efficiency and, with the proper organizational framework, will present an integral coordinated pattern.

CHAPTER VIII

THE ORGANIZATION AND ADMINISTRATION OF RESEARCH PERSONNEL

Organizational Requirements

The goals and objectives established for research in an industrial enterprise must be attained by a group of creative personnel, effectively selected, organized, and administered. As Brown puts it, "Administration is the endeavor of the members of an enterprise to attain or accomplish its purpose, and organization is the act of defining the responsibilities of the members . . . and the relations between them."¹ The organizational pattern should be established upon the basis of the resources of personnel, equipment, and method required to solve the problems selected by top management as being of greatest value to the concern. The organizational framework must be staffed, and these staffs must be directed. This direction constitutes the research management. Livingston points out that

The routine duty of management is integration, and integration is the dynamic aspect of coordination. Coordination is the design of the system for cooperation, integration is the operation of that system. To obtain any result there must be a mechanism but there must also be a personnel, and because in any association there are a whole series of coordinated groups, each of which in turn consists of many different people doing different or sequential things, it is clear that their actions must be integrated.²

The various patterns of activity which we shall consider must each accomplish the following: (1) define the duties and functions of all persons, (2) outline the extent of authority, and

¹ A. Brown, *Organization* (New York: Hibbert Printing Company, 1945), pp. 267ff.

² R. T. Livingston, *The Engineering of Organization and Management* (New York: McGraw-Hill Book Company, Inc., 1949), p. 84.

(3) establish efficient channels of contact between personnel. *In a research organization, the productive unit is the creative mentality. The mode of production is some form of insightful behavior leading to the solution of problems. The efficiency of the results may be measurable in terms of the correctness of these solutions, and correctness is perhaps related to the researcher's systems of arriving at inferences and drawing conclusions from data and information collected.* Each of the productive units will have characteristics different from every other, based on personality, training, and experience. Clearly, it would be desirable to set up the research group in such a way that the most applicable units are utilized for the solution of particular problems.

Of the organizational requirements listed above, the most important is probably the provision of efficient channels of communication. Problems are rarely solved by one person alone. The transfer of such information and stimuli between individuals and groups which will enable more than one creative mentality to be used for a problem's solution is basic to our concept of collective industrial research. We shall examine various types of organizational arrangements from the standpoint of these particular objectives and requirements.

In each of these types of organization an individual or group is given the responsibility for obtaining the solutions to problems. Until we reach the level of the productive unit—the researcher at the laboratory bench or in the field—this responsibility implies the necessity of organizing and managing a group of these units so as to attain an efficient level of output. Clearly, the researcher cannot be directed to create. His efforts can only be reinforced and sufficient incentives provided by proper direction to call forth his best work. This holds true for organizations in general, and particularly where creative behavior is involved. Livingston states in this connection: "The ideal situation exists where the leadership is such that people not only obey orders but attempt to do more than is expected of them. Modern management should attempt to provide the environment in which the creative impulses of everyone within the association are released."³ Perhaps, as Ives points out, "the true research de-

³ *Ibid.*, p. 183.

partment never runs smoothly; it is never satisfied; it never feels that the ultimate in perfection has been reached. . . ."⁴ We still have the obligation to reconcile all the elements in this type of activity to provide the most effective problem-solving organization.

Centralized and Decentralized Organization

As far as the internal framework is concerned, it is not particularly important whether the research organization is physically centralized or distributed among the various manufacturing activities and operating departments, if the proper resources are provided in each location to resolve the problems of that activity. However, the requirement that the proper resources be provided may be quite costly, and a central group utilizing the services of competent specialists may prove more economical. On the other hand, decentralized research activity will probably have a different set of external relationships from those of a centralized organization. These are concerned in general with questions of authority for and selection of the research program. The more centralized the activity, the more closely this responsibility and authority approach top management.

A research director, whether of all the research activities of an enterprise or of one of a group of decentralized laboratories, should have the primary responsibility to attempt to solve all the problems turned over to him.⁵ This responsibility should be delegated to him by his superior in management. Depending upon the enterprise and the nature of the problems selected for research, this superior may be the president, an officer in charge of research, production, engineering, sales, etc., a plant manager, or managerial executive. The exact choice of this executive, to whom the director will report, should be made on the basis of the general over-all policy for research. This policy should be established by top management of the concern and will determine the

⁴ C. Q. Ives, "How Research Differs," *Paper Industry and Paper World*, January, 1947.

⁵ He should already have passed upon the technical feasibility of these problems. Therefore, if he has previously indicated that a problem is unlikely to be solved with his resources, he should not be held responsible for achieving its solution.

scope and type of activity to be pursued. The director should have *formal* authority commensurate with his responsibility. If the policy has been established that the research group is responsible for the solution of problems relating to the entire enterprise, including new products, technical service, production methods, and others, then the director should report to a superior whose status is equivalent to the managers of the other divisions mentioned. Only thus can the requirement that the scientific viewpoint be represented in the selection of problems assuredly be met. In a large company, this superior will very often be a vice-president in charge of research or a technical director. In a smaller company he may well be the president or another company officer.

If the type of problems to be solved by the group is restricted to a particular phase of activity, such as those of one plant or process, then the director may well report to a plant manager or his equivalent. Such a restriction of responsibility and corresponding authority signifies a similar limitation of the results to be anticipated. A department which is responsible through its director to a plant manager for the solution of production problems relating to going processes is not organizationally equipped to develop completely new products and processes. Criteria and judgments applicable to the choice of the over-all research program are useful in establishing the most desirable relationship between research direction and management. *In general, the more closely allied with top management, the greater is the probability that the research direction will carry out desired policies.*

It is a principle of sound organization that "supervision of a member of an enterprise may be exercised by his principal and by no one else."⁶ In decentralized research organizations reporting to a single director, this is often neglected. If the groups are established at several plants and are responsible for the location and correction of troubles in the production process, it is natural for the plant managers to assume control of this aspect of their activities. If this is allowed to occur, it may soon be found that the central management of the department has lost its *de facto* authority to see that the wishes of management are

⁶ Brown, *op. cit.*, p. 257.

carried out. The plant research group will become a trouble-shooting and quality-control organization for the plant manager. These functions may indeed be important and may properly be a part of the duties of the plant staff. On the other hand, if other types of research are desirable and if they are to be done at the plants, then the plant manager should have no supervisory authority over the research group.⁷ It may be desirable either to provide him with a staff for handling his particular problems, not responsible to the central research authority, or to forego the centralized management pattern and have the resident director report to him. Whatever the arrangement, coordination and cooperation will be necessary, and these must be provided by general company administration.

It is not proper to partition responsibility and authority. If a research director is given the responsibility to solve a particular problem and if the problem is of such a nature that it requires the resources of several groups, he should have the requisite authority to direct the work of these groups. This refers to the entire problem. If it is possible to define clearly a part of the problem and place the responsibility for its solution in the hands of another group, then the over-all authority will not be needed. The important point to recall here is that research is problem solving, and that each problem defines the resources needed for its solution. The authority for the direction of these resources should be in the hands of the person responsible for obtaining the desired solution. This is the essence of good research organization. It is not always easy to accomplish, as we shall see.

Subject Type of Research Organizations

Research activities may be organized on a subject, functional, or problem basis. In a small enterprise in which the department consists of only a few workers, all three types of organization may be included in the one group. Our analysis will be based on the larger company where definite divisions are required for

⁷ The resident director may be responsible to the plant manager for maintaining his group in accordance with the established rules of plant administration. Of course, this is relatively unimportant in comparison with actual direction of the group's work.

purposes of supervision, allocation of responsibility, and the reporting of progress. It is applicable to the smaller units as well, if it is remembered that the various activities which we shall discuss as distinct *may* be performed by the same individuals.

In the department organized on a *subject* basis, various groups are established consisting of specialists in the several branches of science. The particular subjects to be included in a given organization will depend upon the nature of the enterprise and the type of problems to be solved. The divisions arise naturally from the separate disciplines in vogue at the academic institutions training these specialists. The variety of such groups may be quite extensive, a typical breakdown being physics, chemistry, metallurgy, mechanics, thermodynamics, hydrodynamics, applied mathematics, electrical engineering, electronics, and mechanical engineering. These groups themselves could be broken down further into component divisions, as for example, physics into physical chemistry, nuclear physics, physics of the solid state, etc.

A subject-type organization is relatively simple to establish and to administer if the problems are clearly separable as to subject matter. This is perhaps most often the case where the studies to be undertaken are of such a nature that their *industrial* objectives have not been clearly defined. Much exploratory research is of this nature. In the operation of this type of organization, a problem is generally turned over to that group in which the major portion of the problem elements seem to fall. The head of the group is made responsible for the solution of the problem and can use the services of other groups on a service or consulting basis, but with no direct authority over them.

Or, the problem may be broken down into subproblems, and these may be parceled out among several groups which seem best fitted for their solution. In this case, the over-all responsibility for the solution and authority for its direction must rest with whoever directs all the subject groups involved. It is not possible to place the entire problem in the hands of one group, and then have the leader of this group direct the efforts of other groups which are equal in the status hierarchy established by this mode of organization. This type of organization is not particularly efficient for administering industrial research depart-

ments, unless all the problems are inherently *one-subject problems* or are very similar.

DIFFICULTIES OF SUBJECT-TYPE ORGANIZATION

A subject-type organization is analogous, in a sense, to a metal-working organization in which all the machine tools are grouped into departments by type. Then, if a specific job requires most of the operations to be done by one machine, say, a lathe, the responsibility for the entire job would be placed with the head of this department. It would be necessary for the preliminary lathe work to be performed, and operations attempted which perhaps could be done more efficiently on some other type of machine tool. If milling operations were required, such work would be done by request to another department. If this situation were strictly similar to research work, subsequent operations would be determined by the results of those done by the milling machine department. When the job was returned, dimensions, tolerances, and clearances might have to be changed. The process would continue in this manner until the job was finally completed.

Of course, no such inefficient procedure would be followed in a job machine shop. The operations required would be determined in advance and the job routed in accordance with a definite schedule from department to department, the over-all responsibility for its completion residing with a central authority. If research work could be so scheduled, then such a procedure could be efficiently followed. The very nature of research precludes such a definite schedule, as would be the case in a job shop if each operation were to change the nature of the subsequent operations in an indeterminant manner.

Some of the difficulties involved in this type of arrangement may be overcome by transferring personnel from group to group as required. However, the establishment of status and the limitations on flexibility make such transfers generally possible only for relatively inexperienced researchers. A senior scientist in one group would not likely look with favor upon his transfer to another subject group. The ubiquitous professionalism of the

scientist is fostered in this atmosphere, and an aura of unhealthy competition is apt to surround assignments and attempts to solve problems.

Nothing is more disheartening or needless in industrial research than the failure to solve problems because of organizational relationships. As an example, we may consider an instance in a rather large research organization in which three groups were involved in obtaining the solution to a particular problem. Each of these groups operated separately, two reporting to one research director, who in turn reported to a vice-president. The third reported directly to this same vice-president. The functions involved were chemical engineering, chemistry, and mechanical engineering, respectively. The problem which appeared to be very worth while attacking and technically feasible of solution was undertaken by all three groups, theoretically in cooperation, although it was difficult to determine where the ultimate responsibility rested. In any event, after a number of years, it appeared that all three groups were working at cross-purposes, that a great deal of money had been spent, and that no solution was in sight. Information which was needed in a particular group was not provided in an accurate, timely, or significant manner. The desire of at least one of the groups to obtain credit for the solution led to the issuance of reports, which by their optimistic interpretation of results caused the orientation of the other groups toward the problem to be misguided. Eventually the problem was abandoned, the only serious obstacle to its solution having been the difficulty of proper coordination and integration of this type of organization.

On the other hand, the subject pattern has been used with notable success in some instances, particularly where the disciplinary basis is not strictly followed in selecting personnel for the groups. Thus, the Armour Research Foundation is divided into a number of subject groups, but the chemistry group is not restricted to chemists, the physics group to physicists, and so forth. Since projects are undertaken on a contractual basis and are for a definite period of time, it is possible for the Foundation to adjust the size and personnel of each of the groups according to the work projected. The volume of work, over \$3,300,000 in 1948, was sufficiently large to warrant diversified and large staffs

in each of the groups.⁸ There is no reason why such an arrangement should not operate efficiently in a large industrial enterprise whose activities are of a divergent nature. However, this efficiency would arise from the ability of the enterprise to apply the team approach within the framework of a subject-type organization.

Functional Type of Organization

The *functional* type of research organization divides the personnel by end product or process. The following divisions are illustrative of this pattern: product research and development; process research and development; process design; engineering; and general services, which include such activities as analytical laboratories, craft shops, or library. It is necessary that each of these groups have personnel of sufficiently varied background so that the problems encountered can be suitably attacked. For this reason, it is not uncommon to find that such functional groups in turn separate their staffs into the various subject disciplines. Such an approach is not the most desirable, although some of the problems of divided responsibility and authority which we previously noted will not be so serious. It is usually quite simple to determine the functional category into which a particular problem falls, and the assignment of the problem to a given group quite definitely fixes responsibility. For this reason, this type of organization has been rather popular in industry. Voorhies writes,

The type of organization adopted for the laboratory ordinarily carries upward, and the same pattern is followed usually in the provision for supervisory and staff positions in the management. A majority of the participating research groups which the survey covers are organized on a functional basis. Some are mixtures. One company on a subject basis is considering a change to the functional.⁹

⁸ Armour Research Foundation, *Annual Report 1948* (Chicago: Illinois Institute of Technology, 1948).

⁹ D. H. Voorhies, *The Co-ordination of Motive, Men and Money in Industrial Research* (San Francisco: The Standard Oil Company of California, 1946).

Comparison of Functional and Subject Organizations

Each of the organizational patterns discussed has disadvantages, of course. We noted that subject divisions encourage professionalism and make it difficult to affix proper responsibility for the solution of a given problem. The functional type of organization may be so interested in end results with respect to its particular function that the broader aspects of developments in exploratory research may not be utilized efficiently. On the other hand, the latter has the virtue of comprehending the greater part of the general field of science and of being able to accept responsibility for the solution of problems with more adequate authority to direct the resources required. Of the two patterns, the functional seems to offer the greater flexibility, and we have seen that this is an important part of being able to solve research problems efficiently. This type of organization is suitable for both large and small firms, and for both centralized and decentralized research divisions. When combined with the problem approach, which will be discussed next, it provides an excellent framework for both control and operation.

The Problem-team Organization

A divisional pattern, based upon the *requirements of the problems themselves*, is a third type of organization which may be used for industrial research. The divisions are represented by scientific teams selected on the basis of the requirements visualized in analyses of the problems. A theoretical analysis of the research process indicates that the most efficient attack is through the medium of groups chosen in such a manner, regardless of organizational or professional lines. This approach has been successfully used in solving problems of large magnitude, as exemplified in the recent war, and has proved highly satisfactory in many instances in industry.

As an example, the research and development leading to the construction of the world's largest sensitized coating machine was recently described. The products of this machine are used in the printing of blueprints, drawings, photographs,

etc. For years, the lack of stability in sensitized papers made it necessary to ship them as soon as they were made. Consequently, the reproduction paper industry comprised small slow-speed low-production coating machines. When a method was found to improve stability, the opportunity was available to utilize more efficient production units. The evolution of such a machine required research and development work in photographic chemistry, the hydrodynamics of coating at high speed, electrical and electronic control of speeds, tensions and inspection, thermodynamics of drying, as well as in several other disciplines. Three major industrial corporations in these fields supplied personnel for a team to carry out this work. Over 20 scientists and engineers worked for some 2 years in this group until the first such machine was completed, to produce up to 12 miles of sensitized paper in 1 hour.¹⁰ Without such a complete team operating under a single project supervisor, it is doubtful that such a development could have been accomplished.

As Williams states, "*So long as clear lines of authority and definite assignments of responsibility are maintained, the freedom to use ability where it is most needed, is advantageous.*"¹¹ In our earlier analogy, concerning the production of work in a job machine shop, the efficiency which would accrue if the machines needed to produce a given item were grouped in one department, with competent personnel under a single authority, should be quite apparent. Of course, machinery is not easily shifted, and even if it were, the ability completely to lay out the operations required in a particular instance makes such an arrangement quite unnecessary. The combination of the requisite resources with the assignment of authority commensurate with the responsibility of achieving a solution to the problem gives this organizational pattern definite superiority over the other procedures.

The use of this method is not without its problems. Shifting a given set of personnel from one team to another, with different supervisors, from two-man teams to teams of 20 or more, having a researcher as a leader in one instance and a coworker in

¹⁰ "The World's Largest Coating Machine," *The Waldron Window*, Vol. 3 (October, 1948).

¹¹ D. L. Williams, *Planning of Research and Development Work* (New York: Wallace Clark & Company), p. 16.

another—such problems can make organization and administration quite difficult. The very background and training of the scientist and engineer militate against the successful operation of such a system. As Keys and Brozek point out,

Successful execution of cooperative research requires modification of the competitive work habits which have been fostered by the hyper-individualistic philosophy of life expressed in the traditions of university research [and] . . . a second barrier to interdepartmental cooperation is seen by Dr. May in the positional system within each department; the young scientist has a greater chance of advancement if he saws wood on his own wood pile, follows the traditional paths and works for the greater glory of his department and does not participate in interdepartmental, collaborative projects. The third barrier is formed by a set of traditional attitudes and ideas such as the belief that scientific discoveries are always the products of individual minds, or that the cooperative setting limits the freedom of the scientist to follow the dictates of his own intellectual curiosity. The fourth barrier is a result of university training in individualistic work habits.

Thus the young scientist . . . is poorly prepared to participate in the activities of a committee or a research team.¹²

These are problems of organization and administration and most emphatically point to *the need for administrative and organizing competence in industrial research direction*. Technical ability is not sufficient; an understanding of the research process and social psychology are probably of greater importance. As the authors quoted above conclude, "Cooperative work is a social art and has to be practiced with patience. A team of research workers representing various disciplines can be welded into a fully integrated unit only on the basis of extensive experience of working and thinking together."¹³ M. J. Shear, in his excellent article, "Teamwork in Scientific Research," from which the above quotations were taken, clearly indicates the recognition that such an approach is the most efficient method of problem solving on the part of such industrial research leaders as Dr. Thomas Midgley, Jr., and Dr. Frank B. Jewett. He says,

¹² A. Keys and J. Brozek, "General Aspects of Interdisciplinary Research in Experimental Human Biology," *Science*, Dec. 8, 1944, pp. 507ff., quoted by M. J. Shear, "Teamwork in Scientific Research," *Personnel Administration*, December, 1946, p. 7.

¹³ *Ibid.*

This delicate flower, cooperation in research, can be planted, nurtured, and protected. There are those who realize how much more effective in research a team can be than the sum total of the efforts of the same people working separately as rugged individualists. But this type of venture is so recent a development that blueprints are not available for those who would wish to benefit from the experiences of others.

For a successful team the first prerequisite is a number of investigators who voluntarily associate themselves in a working group because of the conviction that this is a more fruitful way of working than by playing a lone hand. In the interdisciplinary type of team it is necessary to have at least one mature scientist from each of the specialties involved, and to operate as a democratic working group. The strategy is formulated through discussions among the leaders of the different aspects of a joint problem, rather than being handled by a Boss Man.¹⁴

Of course, Dr. Shear is not speaking directly of industrial research, but there is no reason why the same tenets cannot be applied. The economic factor enters into the industrial situation and creates certain practical problems of organization which would ordinarily not exist in the voluntary institutional team of which he writes. It is necessary that personnel be assigned to teams, not in accordance with individual desires (although these should be taken into account), but as determined by analyses of the problems which management has adjudged the most worth while. The industrial research worker must be led to understand that it is far more to his benefit to solve problems which management considers important than to solve those on which he personally would like to work but which are of little or no value to the enterprise.

Under this system, the research organization will be in a state of flux. An organization chart of its activities at any given time would show several groups, of varying sizes, designated by the problems on which they are working. A similar chart drawn up at some other time might show a different set of groups, with different group leaders assigned to entirely different problems. It might be advisable, although not entirely necessary, to have groups working on related problems reporting to a functional supervisor. Such an arrangement would generally arise in the larger research organizations because of the limitations of the

¹⁴ *Ibid.*

control span of individuals. It is of little consequence, if the personnel necessary to solve particular problems are assigned to working groups, whether these groups report to supervisors in accordance with general functional or subject divisions. In the small enterprise with one, two, or three project teams, such an arrangement is clearly unnecessary; in the larger concern with a larger number of these project teams working simultaneously, some type of division for control purposes will be advantageous. The combination of the problem-team organization with one or the other of the arrangements described above makes for the most efficient work pattern.

Inception of Problem-team Organization

This pattern cannot be introduced into a laboratory or other type of problem-solving group without careful advance planning. It is necessary to visualize the over-all research policy and then consider the particular program to be undertaken. Personnel must be selected to accomplish this program and then divided into the project teams. Some classification must be given to the researchers, so that they may be available for selection as project leaders, and so that incentives for advancement are recognized. It should be possible, as is done in some laboratories and in all military organizations, to designate certain ranks or classes of workers. The nomenclature of the titles is of little importance, Researchers, grades A, B, and C, perhaps being sufficient, or Assistant, Associate Scientist, Scientist, Senior Scientist. These designations should not necessarily be followed in salary payments, although some correlation is inevitable. The top rank should be reserved for those who are considered fitted to act as project leaders—and not all research workers are, some of the most technically competent being poor administrative and coordinative heads. If it is clearly established that the personnel so titled are those who will be project leaders, then no difficulty is to be anticipated in establishing the various groups under their leadership. The remaining titles would be particularly for prestige and incentive purposes. For reasons of morale, it may be advisable to establish a category senior to the group leaders in technical prowess, but not a part of the line of administrative hierarchy. Personnel in such a category, who would comprise

the senior scientists (not desiring to be, or considered qualified for, project leadership) could then be assigned to work under project leaders without loss of status. Each organization and, for that matter, each project will be a case unto itself and will require careful analysis in order to arrive at the most suitable specific arrangement.

Comparison of Problem-type and Subject-type Organization

The objection may be raised that it will be quite difficult to choose personnel in advance of the establishment of specific project teams, and then have the proper number available for continuous operation of the unit. The completion of a project would leave a group without work, or if other problems were available, this group might not be the most suitable to solve any of them. This is recognized as a difficulty, since it is often not possible or desirable to obtain personnel on a short-term basis for the purposes of achieving the solution of a specific problem. On the other hand, an equivalent problem exists in any of the other forms of organization, but the inefficiencies involved are more difficult to discern. Thus, if three projects require a certain number of personnel, these might be divided as shown in Table IV under the problem and subject patterns.

TABLE IV. RESEARCH ORGANIZATIONS

| Problem-type Organization | | |
|---------------------------------------|-----------------------------|---------------------------|
| Project No. 1 | Project No. 2 | Project No. 3 |
| 2 chemists | 1 chemist | 3 mechanical engineers |
| 2 chemical engineers | 1 physicist | 2 electrical engineers |
| 1 mechanical engineer | 1 mathematician | 1 physicist |
| 1 biologist | | |
| Subject-type Organization | | |
| Chemistry and chemical engineering | Physics and mathematics | Mechanical engineering |
| 3 chemists | 2 physicists | 4 mechanical engineers |
| 2 chemical engineers | 1 mathematician | Projects No. 1 and |
| Projects No. 1 and No. 2 | Projects No. 2 and No. 3 | No. 3 |
| Electrical engineering | Biology | |
| 2 electrical engineers | 1 biologist | |
| Project No. 3 | Project No. 1 | |

The solution of one of these problems under either type of organization would leave the same number of research-hours available, if the two operated with equivalent efficiency. Under this assumption, it would be necessary to rearrange the assignment of work to utilize the available time. In the first case, new project teams could be set up from the available personnel if projects on the agenda fitted the abilities of the group. Otherwise, the researchers could be assigned to working groups where more intensive efforts might be desirable. Exactly the same situation would occur in the subject-type organization, but the availability of the additional personnel would not be so clearly apparent, and the unconscious tendency might be to slow down rather than to intensify work.

Another administrative problem may be considered as a difficulty in the problem-type system: a single problem might not require the entire efforts of a given group and their talents might be more efficiently used on a variety of problems within some other type of organization. Thus, it may be that the four mechanical engineers in the two hypothetical organizations outlined above could work effectively on more than two problems at the same time, that there will be delays in a particular project during which other work could be effectively accomplished. In some instances, this objection may be valid, but there are definite compromises with the system which may be used to overcome the difficulty. For example, the size of the particular problem group should not necessarily remain constant but should depend upon the phase and continuous reanalysis of the problem. If the work does not engage all the efforts of the group, then the group should be smaller or the problem should be expanded. If it is felt that the group can profitably undertake more than one project, then these projects specifically should be taken into consideration when the composition of the group is determined. Such variants of the pattern are to be expected and will depend upon the particular enterprise, its research program, and the administration of this program. Flexibility is quite as important in the organizational framework as in the accomplishment of the work itself.

Selection of Personnel

Whatever type of organization is chosen, personnel must be selected to staff it. The over-all size of the research group can be determined upon the basis of the general policy management decides to pursue. Since the task of the group is to solve problems, personnel equipped to do so must be employed. Further, this personnel must have the abilities to attack the type of questions inherent in these problems. For example, it would be manifestly absurd to employ physical chemists to handle problems involving the development and design of mechanical process equipment. Therefore, the first point in the staffing of a research organization is to determine the number and type of personnel which will enable the program to be properly attacked within the limits of expense established by management. A very rare quality is being purchased, and there is no great virtue in economizing here. A research worker who cannot solve problems is of little value, no matter what his salary is, as has been noted previously. For this reason, it is better to reduce the scale of the program, if it is not possible to employ all the personnel required within budgetary limits, rather than to attempt to undertake all the work by means of the employment of less competent and less expensive personnel. The basis for determining the size of the staff should be the ability and desire of the personnel to solve problems, rather than their salary requirements.

It is not possible to establish in advance any optimum or average salaries which should be paid to the several grades of research workers. The salaries for the best personnel in the various disciplines will be quite different under the influence of geographic, general economic, and academic factors. On the other hand, it is not difficult at a particular time to obtain estimates of the going salaries, the professional societies being an excellent and readily available source of information on this subject. In establishing the number of workers to be employed, it should be remembered that for each professional researcher, an investment in facilities will be necessary, and a certain number of assistants and clerical personnel will be required. Some average figures are given by Voorhies, which may be useful in

obtaining a general picture as to the personnel obtainable under a given budget:

The participating companies will have invested in excess of \$6,000 for buildings, facilities and equipment for each research employee. On the expense side, budgets provide, on the average, for an annual expenditure of \$11,000 for each technical worker, or \$5,000 for each research employee, both technical and non-technical. For each technical employee, the participating companies provide an average of one and four-tenths non-technical employees.¹⁵

There are various criteria which may be applied to the selection of the research worker. These include both personality and intellectual traits, as well as experience in a particular field. Walker lists the following fundamental requirements of an engineer, which are applicable to any scientific worker in industrial research:

1. *A general understanding* of technical, scientific and engineering principles.
2. *An ability to use* these fundamentals in the solution of specific problems applying to the industry involved.
3. *The ability to explain* these solutions to other people, clearly, accurately and correctly either in writing or orally.
4. *The ability to obtain acceptance* of these solutions from other people.¹⁶

Many specific characteristics can be listed, particularly with regard to personality, but these are often little more than names applied to desirable aspects of a well-rounded integrated individual and are of little value in actually selecting personnel. It is very difficult to ascertain the character of an applicant with regard to such traits as are found in the following list which has been offered as desirable: curiosity, imagination, experimentalism, enthusiasm, patience, persistence, faith, courage, common sense, honesty, and modesty. Perhaps judgments in these respects may be reached after the worker has been under observation for a long period of time, but certainly not on the basis of one or two interviews.

¹⁵ Voorhies, *op. cit.*, p. 53.

¹⁶ H. C. Walker, "Engineering Training," *Southern Power and Industry*, September, 1945, p. 73.

It is possible to have competent psychologists set up and administer personality tests, which can indicate general aptitudes for specific types of work, and attitudes toward general cooperative situations. Such tests have the merit of eliminating the poor candidates and the danger of eliminating the very best. There is no sure method whereby the selection of only good personnel is guaranteed, but the importance and cost of the creative mentality in the research process warrant the use of the best available means. Consultants are available to indicate the potentialities of a candidate with regard to group work. If used, the results of their studies should only be considered as indicative. If the organization is large, a specialized group may be set up to screen and select promising workers for further interview. In a smaller organization this may be done by the research manager himself. In any case, the final selection should be done by a research administrator who is familiar with his own organization and with the requirements of the program.

Requirements for Research Personnel

The technical requirements vary with the enterprise and the problems being attacked. The author has found that previous experience and scholastic records cannot be depended upon to single out competent research personnel. We would advance the thesis that what is required is a sound background in scientific thinking, professional curiosity, imagination, ingenuity, and an ability to work cooperatively. These follow from an analysis of the methods of research and of the mental attributes of problem solving. We have found that relatively simple tests, to be given to all prospective workers regardless of previous experience or background, are fairly reliable in determining the degree to which a candidate possesses the first four of these. The tests consist of problems so chosen as to require only the simplest of answers based upon fundamental principles, but so worded as to require also imagination and ingenuity in arriving at the desired answer. The problems should be sufficiently varied and numerous, so that chance blind spots on the part of the prospective employee will not affect the result. The exact wording of the test questions will vary depending upon the scientific field and the type of work required. Such a test can be most helpful after

a preliminary interview has indicated the general desirability of the candidate. Four or five problems can serve as a basis for a general discussion indicating ability to translate thoughts into words, both orally and in writing, as well as the general scientific background of the applicant.

In setting up such tests the outline of critical requirements for research personnel which were the result of a study made by the American Institute for Research¹⁷ can be of considerable value. Not all of these are usable for initial interview tests, but they all can serve in evaluating and rating the abilities of the personnel during probationary and subsequent periods. The following items, which were found to be critical, can serve as a check list for administrators, supervisors, and interviewers:

1. Formulating problems and hypotheses
 - a. Identifying and exploring problems
 - b. Defining the problem
 - c. Setting up hypotheses
2. Planning and designing the investigation
 - a. Collecting background information
 - b. Setting up assumptions
 - c. Identifying and controlling important variables
 - d. Developing systematic and inclusive plans
 - e. Developing plans for the use of equipment, materials, or techniques
 - f. Anticipating difficulties
 - g. Determining the number of observations
3. Conducting the investigation
 - a. Developing methods, materials, or equipment
 - b. Applying methods and techniques
 - c. Modifying planned procedures
 - d. Applying theory
 - e. Attending to and checking details
 - f. Analyzing the data
4. Interpreting research results
 - a. Evaluating findings
 - b. Pointing out implications of data
5. Preparing reports

¹⁷ American Institute for Research, *Critical Requirements for Research Personnel* (Pittsburgh: American Institute for Research, 1949).

- a. Describing and illustrating work
- b. Substantiating procedures and findings
- c. Organizing the report
- d. Using appropriate style in presenting report
6. Administering research projects
 - a. Selecting and training personnel
 - b. Dealing with subordinates
 - c. Planning and coordinating the work of groups
 - d. Making administrative decisions
 - e. Working with other groups
7. Accepting organizational responsibility
 - a. Performing own work
 - b. Assisting in the work of others
 - c. Subordinating personal interests
 - d. Accepting regulations and supervision
8. Accepting personal responsibility
 - a. Adapting to associates
 - b. Adapting to job demands
 - c. Meeting personal commitments
 - d. Being fair and ethical
 - e. Showing interest in work

Retention of Research Personnel

In addition to selecting personnel to work in a research organization, it is necessary that the personnel be retained and given the opportunity to experience satisfaction with their work. The survey carried out by the National Opinion Research Center for the President's Scientific Research Board is probably the most definitive study available concerning the satisfactions of a career in science. This survey found that of industrial scientists, some 42 per cent expressed the opinion that more satisfaction was obtainable in other than industrial work.¹⁸ This indication of a degree of dissatisfaction was related to age, but not to income; *i.e.*, more of the younger professional workers were apt to be dissatisfied, but the percentages were approximately the same for all income groups. It was also found that

¹⁸ J. R. Steelman, "Administration for Research," Vol. 3 of *Science and Public Policy* (Washington: U.S. Government Printing Office, 1947), Appendix III, pp. 205ff.

the longer the employment experience in a given position, the more likely was satisfaction to be expressed, which is indicative of the fact that those who are dissatisfied with their work are apt to change positions voluntarily or be released. Therefore, it is most desirable that attention be given to the creation of satisfactions for the newly hired worker, since a good start in this regard will carry a great deal of weight in his future career. It should be apparent that turnover of personnel in this type of work is not efficient, especially when it is recalled that the creation of a collaborative working team requires time and careful effort. Accumulated experience with the particular problems and processes of an enterprise is worth a great deal in solving its problems efficiently.

In analyzing the results of this survey to determine what industrial research management can do to increase the satisfaction of the individual worker, Table V is outstanding. It relates to the special satisfactions in a scientific career, in contrast to other occupations.

TABLE V. SPECIAL SATISFACTIONS IN A SCIENTIFIC CAREER *

| | |
|--|---------------------------------|
| I. Allows scientist to do the kind of work he wants in the way he wants: | <i>Per cent of total sample</i> |
| A. Intellectual and temperamental satisfaction: | |
| 1. Learning how things work..... | 31 |
| 2. Variety, discovery, challenge..... | 22 |
| 3. Creative satisfaction..... | 9 |
| B. Tangible results..... | 15 |
| C. Independence, initiative..... | 7 |
| D. Depends on individual's taste..... | 9 |
| | — |
| Total..... | 93 |
| II. Other satisfactions: | |
| A. Social values of the work..... | 28 |
| B. Human contacts, environment..... | 5 |
| C. Prestige, recognition..... | 3 |
| D. Economic rewards, security..... | 1 |
| E. Don't know, vague..... | 5 |
| | — |
| Total..... | 42 |

* J. R. Steelman, "Administration for Research," Vol. 3 of *Science and Public Policy* (Washington: U.S. Government Printing Office, 1947), Appendix III, p. 211.

The variety and novelty of its problems are obviously viewed as important assets of industrial science. Although economic rewards are not mentioned as being of great importance, nonetheless, a later table indicates that some 80 per cent of industrial scientists feel that the various rewards—financial, prestige, and otherwise—that accrue to scientists in America are less than they should be. Income level has little relation to the scientists' opinion about the adequacy of rewards.

Moreover, none of the questions relating to direct job satisfaction shows considerable correlation between opinion and income level. The lack of clear-cut correlation between income level and attitudes is in itself a significant finding. It suggests that opinions about the inadequacy of money and prestige rewards are a matter of fact rather than emotional in tone. Comments made on this point also suggest that it is not a focus of strong feeling.¹⁹

The results of this very complete survey should be utilized by all research management in evaluating the satisfactions their own organizations offer to the professional worker. It is notable that the scientist obtains his satisfaction from exactly those accomplishments which are necessary if a problem-solving group is to operate efficiently. As Appleton points out: "As for incentive, we must always remember the enthusiasm and the challenge of the scientific chase. My experience is that science, whether the objective be a greater insight into some natural phenomenon, or some more practical task, can be an adventure as well as a job."²⁰ Seventy-seven per cent of the sample listed above indicate their satisfaction as coming from one or more of the following: variety, discovery, challenge, learning how things work, creativeness, and attaining tangible results—in other words, from participating in the research process itself.

A definite policy should be established to furnish the opportunity to gratify these desires, particularly among the newer and younger staff members. The problems to be solved should be presented in a stimulating manner, and it should be an established practice to have each member of the organization understand the scope of the problem and the anticipated results.

¹⁹ *Ibid.*, p. 218.

²⁰ E. Appleton, "The Scientist in Industry," *Chemistry*, Vol. 22 (August, 1948), p. 45.

Probably nothing can so detract from the possibility of achieving each of these gratifications as the piecemeal assignment of a part of a problem with a method and procedure fully outlined, leaving little for the individual to accomplish with his own imagination. Authoritarianism in this type of work can lead only to dissatisfaction and inefficiency. This is another argument for the team approach, in which all its members are fully familiar with the problem and can share in the satisfaction resulting from its solution. A highly organized, rigid structure, which might result from a subject type of organization, can hardly lend itself to such a policy. The other important factor in maintaining a high degree of work satisfaction is that resulting from the social values of the work. It should be possible to indicate to the researcher just what his problem solving will attain in the way of increased production, quality, or lowered prices. Of course, if no such results are anticipated, such a point will be difficult to make.

Salary Problems and Job Analyses

From our standpoint, the research group should consist of above-average personnel, who are paid above-average salaries. In general, each man should be rated separately, and his accomplishment evaluated in terms of intragroup effort, conscientiousness, and the character of the results he has obtained with relation to the difficulty of the problems worked on.

Job analyses of the various positions in the organization may be undertaken if the size of the group warrants the expense. It must be remembered that evaluating professional levels and establishing rate of pay levels is a complex task and one which must be preceded by extremely careful analysis. Any such plan should take into consideration such factors as (1) education and experience, (2) supervision required and (3) received, (4) exercise of independent judgment and initiative, and (5) responsibility for attaining solutions to problems. Based on these factors and others which may be applicable in a given instance, a rating system may be established and salary curves set for various levels. It may be expedient to have minimum and maximum curves, to allow for individual differences, and seniority advancement. Such a system must itself be subject to constant

analysis, and care must be taken to see that changed conditions do not upset what may have been an equitable pattern.

Salaries should be increased regularly, but the regular payment of individual bonuses for problems solved is not a sound practice. If the bonuses are high, as they were in the I. G. Farben-industrie during the war,²¹ the system is apt to lead to jealousy, secrecy, and lack of cooperation. Where the team approach is utilized, some type of team bonus could be evolved which might be satisfactory, but if this in any way penalizes teams with difficult problems it should not be used. The establishment of a group bonus system for the entire research organization has considerable merit in improving morale and adding incentive. The amount of this bonus should be based upon the accomplishment of the entire group, and all members should participate. Individual payments may be made on the basis of status or length of service, or both. If the employees are paid above-average salaries, they will know it, and no further incentives of this type are required. It is more important to recognize merit by encouraging the publication of papers, the obtaining of patents, attendance at scientific meetings, and increase in status than with financial rewards (if these rewards are adequate to begin with).

Professional Development Opportunities

The continued development of the abilities of research personnel should play an important part in the administration of this type of work. This task really begins with the universities whose fundamental role is

to discover creative talent and provide for its education and training. The process must be carried through the graduate schools until the product is ready to man the research laboratories of the nation in the work of creating and applying knowledge for useful purposes.²²

However, we are here concerned with what can be done in industry to continue this development and shall assume that the workers have been properly selected on the basis of previous

²¹ This is reported to have been 5 per cent of the sales return on a product for which the worker held a patent.

²² F. L. Hovde, "The Universities' Role in the Nation's Research," *Chemical Engineering*, Vol. 56 (January, 1949), p 105.

training and other factors. Can the creative mentality be developed further and trained in industry? It is generally conceded that it can. Veal writes,

Can imagination be developed? As in the case of every other quality, physical or mental, a person is limited by his natural endowments, but within these limitations the possibilities for development are far beyond those ordinarily achieved. Imagination, like an organism, grows by food and exercise. The food of imagination is perception. The mind must be stored with impressions seen and assimilated by the individual, not absorbed through textbooks. For true richness of imagination, impressions should not be restricted to the field of immediate utility. The mind should be kept open. . . . In the exercise of imagination, solitude and individuality are essential. A man must explore the resources of his mind, free his thought from convention and the fear of non-conformance.

The imagination must be disciplined as part of its cultivation. . . . An undisciplined imagination makes a Jules Verne; a disciplined imagination, a Leonardo da Vinci, Goethe, or a Steinmetz.²³

The attributes of the scientific method and the use of creative insight should be recognized and encouraged in the research organization. By making certain that significant results are recognized as being significant, indicating and providing examples of the means whereby these results may be achieved, management can do much to develop the use of the systematic manner of problem solving. By giving the researchers the opportunity to add further knowledge, particularly in fields unrelated to their academic disciplines, their ability to solve the problems presented to them will be enhanced. The procedures whereby this may be done include group meetings where members of the organization explain the background and nature of the problems of their own divisions or teams. The provision of libraries and adequate resources to obtain information and to study in a relatively quiet and restful atmosphere can assist in decreasing the time required for "problem incubation." Visits to production units and studies on the ground are also useful. It will be noted that all types of activities designed to promote development of the creative processes are also convergent with those which enhance

²³ C. B. Veal, "Research—Opportunity and Challenge," *Mechanical Engineering*, Vol. 69 (September, 1947), p. 760.

the satisfactions the research worker would like to obtain from his occupation.

Research Leadership

Research activity cannot be "directed" in the usual sense of establishing procedures to be followed at each stage of an industrial process. As Williams writes,

The techniques of planning which have been developed in the management of Industry are just as applicable to the management of Research and should be utilized there. This becomes obvious if we consider the many points of similarity. In both Research and Industry, money, men, machines and material must be brought together and so managed as to achieve a desired result. The problems of accounting, personnel, maintenance, purchasing, storekeeping and many others are common to both. . . .

The basic difference which should not be lost sight of is that the key productive unit in Research is the scientist, and that in turning out ideas he must be largely his own manager and his own laborer.²⁴

It is this latter point that we have been stressing. It has often been said that such activity cannot be directed except on the working level. Therefore, the important management factors must arise with the leaders closest to this level, who in our recommended organization would be the team leaders. The team is established to solve a particular problem, or in some cases several similar problems. It may be said to operate upon this problem in terms of the creative mentalities and other resources available to it. The relationship of the team leader to other members of his group, and to the problem, may be considered to be in the nature of a catalyst. He should act so as to keep the attention of the team as a whole focused upon the ultimate results desired, to see that desired equipment and services are obtained, to make sure that the individual attitudes of the members are combined into a general attitude, and to contribute to their collective creative mentalities. The very fact of maintaining all the problem vectors in view and of establishing and reestablishing the major objective will assist in evolving the ideas required for the solution.

McLenegan states, "Thus management, by establishing favorable leadership as a stimulus and by combining various

²⁴ Williams, *op. cit.*, p. 5.

points of view in a well-balanced organization, can effectively set the stage for creative work.”²⁵ The relationship of the team leader should be much closer to his group than to his own superiors. He should maintain close contact with all his teammates and actually engage in the work himself. If the nature of the problem is such that the team is too large for this, then it should be divided into smaller groups with working leaders at a lower echelon. The advantage of having a focal point around which the day-to-day problems can be resolved, and through which ideas can be communicated and integrated both within the group and to the entire organization, has been demonstrated by most successful industrial research.

Above the working level, the problem of direction and management becomes one of planning and of providing the general environment in which creative activity is fostered. It should have the objective of providing the actual workers the help they need when they need it. Veal notes the requirements of such management:

In a large laboratory, the research manager . . . must guard against breaking down projects in too minute and simple portions, as in a production assembly line. In such overspecialization, a promising creative research worker might be whittled down to a routine laboratory tester. As manager, he must exert the necessary control and direction, without impeding the freedom of thought requisite for research. The problems of sales and production must be considered, but the viewpoint of research maintained.²⁶

It is the laboratory manager's responsibility to see to it that proposals and going projects are properly evaluated, and that critiques and appraisals are forwarded to management in a timely and intelligible fashion. He will also evaluate and approve the estimates of resources required to carry out a program and will select both the personnel of the research staff and those to operate as a team on a given problem. The task of the manager within a subject or functional organizational framework is much simpler in this respect, since it involves only the choice of a particular established group to carry out the work. The

²⁵ D. W. McLenegan, "Invention in Engineering Development," *Mechanical Engineering*, Vol. 69 (August, 1947), p. 662.

²⁶ Veal, *op. cit.*, p. 759.

creation of new groups for each problem is not an easy task but, if conscientiously done by a competent manager, will pay extra dividends in the form of efficiency and more useful results. The manager should maintain sufficient contact with the research personnel so that he is in a position to select team members and leaders, and so that he may properly evaluate the merits of the workers in the organization. He should provide himself with a competent staff of administrative assistants to manage and administer the necessary auxiliary services to the actual creative work. Then, he will be able to devote his attention to the important factors in achieving satisfactory solutions to his problems.

Management of research at the higher levels in an industrial concern, above the actual direction of the working organization, should be devoted to the planning of the nature, scope, and magnitude of the activity. The necessity for careful analysis and evaluation of proposals and programs should be clear. It should be iterated here that an attempt actually to direct the procedures (whereby the work is undertaken) from this higher level can lead only to inefficiencies and confusion. This would be true whether those in top management are competent scientists or not. The author has witnessed several cases where research effort and time have been wasted in following specific directives on specific problems from highly placed executives. Usable results can be obtained only through work involving a knowledge of the details, and these can never be available at any great distance from the working level. In large organizations, it may be desirable for top management to provide one or more staff executives to be responsible for the general over-all guidance and evaluation of the corporation's research activities in specific areas, such as products, processes, or markets. These executives and their staffs would have no direct authority over the working research organization but would handle the evaluations and appraisals previously discussed.

Services for Research Personnel

The remaining elements in the organizational structure are those providing services and other functions of a nontechnical nature. It should be apparent from the discussions above that

such services should be readily available within the research organization but should not be a part of the actual problem-solving work. This is a matter of economy, enabling the higher priced professional personnel to concentrate their attention upon problems of a technical nature. Such functions as coordination of public relations, clerical preparation of reports, assistance with correspondence, safety, cost, and budgetary control may be included in the framework, under a separate executive, perhaps called the "business manager," or "manager of general services." Among the services which may be so provided are various routine laboratories such as physical testing and analytical chemistry, glass blowing and glass washing, maintenance and craft shops, accounting, library, stockroom, blueprinting and photography, and any others which are utilized by the various project teams in a routine manner. The problems of establishing and managing such services are similar to those encountered in business generally, and the important point to be made is that they are service groups whose function is to provide the maximum in assistance to the working researcher and, in this case, at the minimum cost.

Research Personnel, Professional Societies, and Unions

The research worker, in his professional capacity, has had a long tradition and background in organizing and becoming affiliated with collective groups. These, however, have usually been formed for the purpose of advancing his knowledge and understanding in his chosen field, and not with the intent of entering into collective bargaining negotiations for the improvement of wages or working conditions. He has been highly individualistic in the past. The professional societies have served him well, providing for mutual exchanges of information and opportunities for broadening his contacts among those with similar interests. These groups must be fostered and encouraged by management. Research personnel should be given the opportunity to participate fully in this type of activity for the many reasons already discussed.

A number of collective bargaining groups have been formed by research workers in recent years. Some of these have been local only, but others such as the United Office and Professional

Workers (CIO), and the Federation of Technical Engineers, Architects, and Draftsmen's Unions (AFL) are nationally affiliated. The moral right of professional workers to become members of unions is generally not questioned;²⁷ the economic necessity of their doing so is quite a different matter. Where industrial research has become a large-scale undertaking and where the professionals are not treated as a part of management, or where their needs for creative satisfactions are not met, there is little doubt that they will turn to collective action to improve their status. This action may come through unions or through their professional societies but, in either case, the net results will be quite similar. Management will do itself and its research workers great disservice if the conditions and rewards of research work are not maintained at the highest possible level. Where unions have been formed, managements have found it possible to bargain intelligently and amicably concerning these matters.²⁸ As we have seen, mutual confidence, whether with unions or individuals in research, is of the utmost importance in achieving satisfactory and efficient effort. Antagonism can only hamper and discourage creativeness and, after all, creative results make research in industry pay its way. Therefore, relations with researchers and management should be maintained on the highest possible level of confidence and understanding, whether the personnel choose to unionize or not.

Consultants

The use of consultants should perhaps be noted in connection with the personnel of a research organization. They may be utilized in two ways. In certain cases it may be desirable to retain experts in a given field to advise and consult on the general research program or on specific projects. Such experts would then serve as additional sources of information for management's evaluation and appraisal of projects and proposals. Or, actual research work may be turned over to an outside con-

²⁷ H. R. Northrup, "Industrial Relations with Professional Workers," *Harvard Business Review*, Vol. 26 (September, 1948), p. 544.

²⁸ *Ibid.* There have been examples also of difficult and unsatisfactory negotiations in which it is clear that both the enterprise and the professional workers have lost far more than either could have hoped to have gained.

sulting group when it would prove economical or feasible to do so. Such a group might be a nonprofit institution or foundation, or a private firm of research personnel. The use of such facilities should be considered from the standpoint of economics. A small firm might not wish to establish extensive facilities, or a large firm might not wish to enter a new field without some exploratory work being done to indicate the feasibility of the new field. Supervision of the work will normally be retained by the firm hired, but general policy can usually be mutually decided upon. Arrangements for patents and secrecy should be made in advance. The decision to use such facilities will be determined largely by the general policies adopted by management with regard to research activity.

Summary

In summary, the organization and administration of research personnel is a matter which should be given a great deal of careful attention by management. The researcher must be treated as an individual, and the organizational framework in which he works must be designed so as to combine the authority required to attain the solution to a problem with the responsibility for achieving the result. The most efficient form of organization is probably that in which teams are set up to attack specific problems, the composition of the group being determined by an analysis of the problems to be solved.

Whatever the form of organization, proper planning of requirements and selection of the best available staff, whose members have sound professional backgrounds, curiosity, imagination, and ingenuity, as well as the ability to work cooperatively, will go far in assuring efficient and noteworthy results. Attention must be devoted to allowing the workers to achieve satisfactions in their work and to feel adequately compensated for what they accomplish. This can be accomplished best by a clear understanding of the professional and personal desires which must be gratified, and by administering the group directly at the bench level, and handling matters of policy only at the higher levels. In this way, research in industry can be made to respond to the necessity for more efficient operation without in any way hampering the creative abilities of its personnel.

CHAPTER IX

RESEARCH ECONOMICS AND BUDGETING

The Background of Research Accounting

In order that the organizational and administrative procedures outlined be efficiently controlled and evaluated, it is necessary that operational standards be established. A measurement of the degree of accomplishment may be then obtained by comparing these standards with actual performance. This entails the construction of categories into which classes of similar and comparable information may be collated, as well as provision for the requisite means of collecting these data. The utility of such basic budgetary concepts has not been commonly recognized in research. This is understandable, since the nature of the variances relevant to much of the information has been such as to make it appear undesirable or unprofitable to attempt their use in forecasting and measurement. However, if the nature of the process and the significance of the estimate and performance data are understood, the measurement of performance is an extremely valuable and, in a sense, an essential part of rational procedures.

It is of little consequence to plan projects and programs carefully, if there is no method established for measuring results in terms of the predictions (implied or actual) which comprised the goals and objectives. The over-all plan of operation for a research organization during a given period of time may be established by means of a budget. Its extent will be determined by the categories of information defined as being comparable. In the very simplest case, and probably the rarest, no metrical data with regard to research activity are considered sufficiently similar to be worth summing in one category, and no budget of any kind is utilized. In such instances, all research expenditures are lumped with general administrative costs. Where this is done, it is usually due to the lack of *significant* (in terms of

profits or sales) expenditures on the part of the particular company for such activity. A number of companies in initiating research work have operated on this basis and have usually found it advisable later to shift to some type of more informational research accounting system.

In the much more frequent case, the total costs of doing research are compared from fiscal period to fiscal period, and some general scale of activity is made an objective of the organization. Costs and anticipated benefits of individual projects are neither estimated nor measured. This single-sum accounting represents a budgetary control and, used in connection with subjective evaluations of results, is a measurement of research accomplishment. We would not wish to state that this type of control is completely without merit; if the research expenditures are quite small in relation to sales or profits, it may be all that is warranted. On the other hand, if the process is to be managed with an objective of optimum efficiency, it is necessary that understandable standards of performance be established and compared with measurements of costs and values of work done.

The decisions of management for a desirable research program should have been based upon the same types of information which may be used to set up a periodic forecast of activity. These data should include:

1. A decision as to the limit of over-all magnitude of research activity, based upon
 - a. The amount which the enterprise can afford to spend
 - b. Research results which are needed
2. Decisions as to desirable projects, based upon
 - a. Estimates of the resources and time required for solving the problems
 - b. The results anticipated
 - c. The technical feasibility of attaining solutions
 - d. The value of the results to the company

With this information concerning the program, a company will be able to determine (1) *how much it can spend* and (2) *when it may expect to have the results which are anticipated*. When these data have been used to set up the program in terms of estimated expenditures and values of the results, they are susceptible to analysis and review. Certain ratios may be estab-

lished which can be utilized to obtain a comprehensive picture of what the program may accomplish and the nature of the anticipated returns in relation to the risks involved. Any predictions of the value of the results should have considered these risks.

The Research Program Forecast and Budget Statement

A generalized form of a program forecast may be set up in terms of this information. Depending upon the particular enterprise, the time period used may vary, from approximately 3 months to a year. Less than 3 months would yield results of little additional significance, even where the project turnover is very high. Periods longer than a year are undesirable because of accounting, taxation, and other fiduciary procedures for which yearly recapitulations and audits are required. In any case, the forecast should include more than one cost period, the exact number depending upon the length of the individual period and the duration of the projects. If the research program has been established and the data are available, past performance data for several periods should be included. These have been indicated in Table VI. We shall later discuss methods of obtaining the necessary information. Symbols have been assigned to the various elements in the table so that they may be manipulated with greater ease in subsequent discussions. In analyzing this type of activity, we are not so much interested in measuring variance above and below our estimates, as we are in estimating progress toward various goals and in determining the nature of the risk taken. Therefore, the program forecast and budgetary performance statement will partake of the nature of predictions based upon economic and technical judgment. These predictions will be subject to random as well as organized errors of judgment. The inclusion of measured data on past performance and results will tend to reduce the significance of these errors for predictive purposes. The nature of the risk assumed must be taken into account by setting up a program in which the value of the results outweighs the possible costs by a sufficiently high factor.

In this table, projects have been designated as A_1, A_2, \dots, A_n , their estimated value to the company as E_1, E_2, \dots, E_n .

TABLE VI. GENERALIZED RESEARCH PROGRAM FORECAST
AND BUDGET STATEMENT

| Item | Project, A | | | | Total |
|--|------------|----------|---------|---------|---------------|
| | A_1 | A_2^* | A_i | A_n | |
| Estimated value to company, E | E_1 | E_2^* | E_i | E_n | ΣE_n |
| Estimated time allowable for completion in periods, T | T_1 | T_2^* | T_i | T_n | ΣT_n |
| Estimated cost for present phase of project, this period: | | | | | |
| Materials, $1m$ | $1m_1$ | $1m_2^*$ | $1m_i$ | $1m_n$ | $\Sigma 1m_n$ |
| Research personnel, $1p$ | $1p_1$ | $1p_2^*$ | $1p_i$ | $1p_n$ | $\Sigma 1p_n$ |
| Overhead, $1o$ | $1o_1$ | $1o_2^*$ | $1o_i$ | $1o_n$ | $\Sigma 1o_n$ |
| Total, $1x$ | $1x_1$ | $1x_2^*$ | $1x_i$ | $1x_n$ | $\Sigma 1x_n$ |
| Estimated cost for project, next period: | | | | | |
| Materials, $2m$ | $2m_1$ | $2m_2^*$ | $2m_i$ | $2m_n$ | $\Sigma 2m_n$ |
| Research personnel, $2p$ | $2p_1$ | $2p_2^*$ | $2p_i$ | $2p_n$ | $\Sigma 2p_n$ |
| Overhead, $2o$ | $2o_1$ | $2o_2^*$ | $2o_i$ | $2o_n$ | $\Sigma 2o_n$ |
| Total, $2x$ | $2x_1$ | $2x_2^*$ | $2x_i$ | $2x_n$ | $\Sigma 2x_n$ |
| Estimated total cost for project, C | C_1 | C_2 | C_i | C_n | ΣC_n |
| Estimated total time for completion of project in periods, t | t_1 | t_2^* | t_i | t_n | Σt_n |
| Actual expenses of project to this period: | | | | | |
| Materials, M | M_1 | M_2 | M_i | M_n | ΣM_n |
| Research personnel, P | P_1 | P_2 | P_i | P_n | ΣP_n |
| Overhead, O | O_1 | O_2 | O_i | O_n | ΣO_n |
| Total, X | X_1 | X_2 | X_i | X_n | ΣX_n |
| Estimated or actual returns from completed projects for r previous periods, or value of project, R | R_1^* | R_2 | R_i^* | R_n^* | ΣR_n |

* Project A_2 is considered completed. Therefore, the estimated values (*) would be zero and not included in the summations. The value of R for incompleted projects would also be zero.

the time allowable for profitable solution as $T_1, T_2, . . . , T_n$, etc. Project A_2 is considered to be a completed project and, of course, in a complete statement we would expect to have more than one completed. However, we are for the present concerned with the over-all picture and are particularly interested in the totals, such as ΣE_n and ΣT_n . The first item of significance to be considered is the estimated cost of the program for the period under

consideration, $\Sigma_1 x_n$. *This sum should be equal to or less than the amount which has been determined by management as the safe limit of expenditures for research for a given period.* If it is greater, then it will ordinarily be necessary to defer work on a sufficient number of proposed projects to reduce the estimated cost to the desirable value. In deciding upon the projects to eliminate, the data in this form can be extremely helpful. It is possible to determine the ratio of estimated value to estimated cost for each of the projects (E_i/C_i), which will establish an order of project rank in terms of potential returns. For the same projects, the ratio of time allowable for completion to the estimated total time required to complete (T_i/t_i) gives an indication of the safety factor estimated for this element of risk. Both of these ratios may be helpful in adjusting the program to acceptable limits of expenditure.

It should be apparent that the portion of the total estimated cost which is represented by research personnel ($\Sigma_1 p_n$) must be equivalent to the salaries of such individuals in the organization during the period. If it is lower, then the work load is too light; if higher, either more will be required or some of the estimated work will not be completed. Adjustments are possible in these estimates by deferring projects or increasing the intensity of the work. When the adjustment has been made, to bring the forecast into line with desirable expenditures and available personnel, further analyses may be made.

Estimates of Risk and Projected Costs

The over-all ratio of the value of the program to estimated total costs ($\Sigma E_n/\Sigma C_n$) determines the estimated risk involved in entering the work. That this ratio should have a value greater than 1 is obvious. The higher the value, the smaller the probable risk of loss if some of the projects are unsuccessful. It should be recalled that a predetermined ratio of estimated value to cost was not recommended in setting appropriations for individual projects. Therefore, each of the projects appearing in the forecast should have estimated value-to-cost ratios in which estimates of feasibility have been taken into consideration. Generally, a standard value cannot be established for this relation-

ship. It will be necessary to determine the safe ratio for each business from a study of past records and check these against future performance. Values ranging from 10 to 3 have been reported, but due to variations in procedures in determining the present worth of the research results, they are not particularly comparable. A lower limit of 2 seems reasonable. This means that, on the average, one out of two projects must be successful if research is not to operate at a loss. Such might be the case where low-risk intraprocess improvements formed the majority of the projects. An upper limit of 10 for this ratio is generally accepted in high-risk high-return industries.

In order to project the over-all expenditure rate for the program, the ratio of estimated costs (to complete) to estimated total time for completion may be determined $[(\Sigma C_n - \Sigma X_i) / \Sigma t_n]$. This value compared to the estimated cost of the period under consideration is an indication of the reasonableness of the projected cost and time estimates; *i.e.*, if this projected value is considerably higher than that estimated for the present period, the costs either have been overestimated in relation to the time requirements, or the program will have to be intensified to meet the time estimates. If the reverse is true, then the program will require new projects or intensified work in future periods if the present rate of expenditures is to continue. This particular consideration may be further amplified by use of the ratio of estimated costs (to complete) to the estimated costs for the period $[(\Sigma C_n - \Sigma X_i) / \Sigma_1 x_n]$. This value gives the number of periods of research work which are available in the present forecast at the rate of expenditure estimated for the period being budgeted. Under most circumstances, the research program should be planned ahead for at least one period, and up to as many as 10, depending upon the industry. The estimated costs for future periods should include at least the number of periods indicated by this ratio. (In the forecast shown here only one future period has been indicated, but this may be expanded to whatever number seems desirable in a particular instance.)

Since research overhead is generally uncontrollable by the project leaders, it may be desirable to remove it from the individual projects ($_{10i}$) and indicate it separately as a total for each period (Σ_{10} , Σ_{20} , etc.). This will serve to make the esti-

mates and ratios *on individual projects* more sensitive to material and personnel costs involved directly in accomplishing the work. Ratios of material to personnel costs and of personnel to total costs have little utility, since each project should have its own peculiar requirements for resources. Average values which might be obtained should not be used for long-term forecasting, since they will be greatly affected by changing economic and technical conditions.

USE OF PAST RESULTS

When actual expenses of projects for previous periods and projects completed within those periods are included in the forecast, it will have a much higher degree of accuracy and usefulness. The total amounts expended prior to the period under consideration should be included, as well as the expenses of projects recently completed. The decision as to how far into the past the list of completed projects should extend will depend upon the nature of the enterprise and the program. An arbitrary but not completely illogical means of determining this value is in terms of the average estimated time of completion of all projects in the program ($\Sigma t_n/n$). Data for projects completed within at least the number of periods indicated by this ratio may be included. This will ensure that the evaluation of research performance in terms of a standard includes approximately equal periods of past and projected time. Of course, if the data are not available for this period in the past, it will be necessary to use what is available until the required records have been compiled.

The inclusion of these values in the determination of our standard ratios makes them much more representative. The completed projects included in this analysis should be both the successful and the unsuccessful ones. *It is important that the estimated value of research results be evaluated in similar terms for both proposed, current, and completed projects.* If they are not, these ratios are meaningless. A number of methods may be used for this purpose. We have proposed a value to be determined by management based on its best judgment of the present worth of the anticipated or realized results. This has the advantage of simplicity and the possible disadvantage of subjectiveness. The determination of the present worth of a pro-

posal or a completed project involves either the savings per period effected by process improvements, or increased net returns from sales on improved or new products. These values may be calculated in terms of net return on capital investment, current interest rates, and capital recovery periods. Standard formulas are available for these calculations. One such formula, known as the Hoskold transformation, is given as

$$P = \frac{D}{R + \frac{R'}{(1 + R')^n - 1}}$$

where P is the present worth of the annual savings or net income,

D is the annual savings or net income,

R' is an average net return on capital investment in the enterprise,

R is the current rate of interest on investments.

Thus, if a process improvement results in a net saving of \$10,000 annually, the business earns 10 per cent on capital investment, the going interest rate is $2\frac{1}{2}$ per cent, and the research and investment costs are to be recovered in 4 years, it will be found that the present worth of this improvement is \$41,590. Such precision of calculation is usually not warranted, but a similar method is useful in reducing the calculated values to a comparable basis. The annual savings or increased net income from sales are, of course, estimated for proposed projects, based on the results anticipated. For completed research, these are still subject to estimate, but of a more accurate variety. Some improvements in product, for example, do not affect the price and, because of competition, may not affect the net sales. However, management can set a value upon these on a comparative, if subjective, basis. Sound accounting practice should be used to arrive at these values, so that management can rely upon them just as they do upon the accounting or production operations. It should be noted that the capital investment required when the project is completed must be subtracted from the present worth of the results in obtaining the value of the research. In the example above, if the capital investment necessary to put the process into production were \$20,000, then the research value would be \$21,590.

A different type of evaluation, proposed by Aries and Happel, establishes a measure of "venture" profit which is distinguished from the profit normally realized in the business. In this method, the profit on a given investment (in research and plant), after taxes, is compared with the normal profit after taxes on the same investment in going operations. Thus, various research projects could be compared upon the basis of their extra return. This method is perfectly valid, if all projects are compared on this basis, and is a way of arriving at the present worth to the company of a given project.

Another method for arriving at these evaluations is described by Olsen:

A good method of accomplishing this end is to present the research division's estimate of anticipated savings of materials, labor, etc., compared with the base-line conditions, prevailing before the new process is introduced, as established by the auditing department. It is then management's direct concern to see that this potential saving is actually earned. Of course, the capital investment needed to secure this saving must be shown in order that the return on the investment can be clearly recognized.

When this improved process is put into operation, the cost accounting department reports, preferably monthly, or at least quarterly, just what saving has been made. This saving is calculated only on the recorded actual shipments of salable product made by this improved process. . . .

These savings are recorded quarterly *for one year only* as a measure or index of the return on the research effort expended on the process. . . .

The practice is to consider three percent of the gross sales of [a] . . . new product for a period of three years as a fair measure of the value of the research.

For *improvements* in products there is recorded three percent of gross sales of that improved product *for only one year*.¹

A method such as this has considerable merit, and the values obtained are quite usable in our proposed budgetary statement. The selection of the period to be allowed for computation of the

¹ F. Olsen, "Evaluating the Results of Research," in C. C. Furnas, ed., *Research in Industry* (New York: D. Van Nostrand Company, Inc., 1948), Chap. XXI, pp. 405ff.

returns, as well as the percentages to be used, is a matter for management decision in a particular enterprise. There is a certain lack of flexibility in fixing these values in this manner, but this may be perfectly satisfactory in industries where improvements and new products may have short lives due to competitive conditions. The present worth method allows management in each specific case to determine the period in which it would like the investment in research and production facilities to be returned. It is possible to visualize variations ranging from 6 months to 10 years, depending upon the nature of the particular product. It is quite simple to translate the methods outlined by Olsen and Aries and Happel to present worth values, and it should be apparent that the additional factors of current interest rates and desirable return on the investment are worth considering. In addition, the changing taxation situation must be taken into account from the standpoints of net return, amortization, and charging off the costs of research.

Taking into account actual returns from completed projects, and *reestimates* of the *cost to complete* going projects based on prior expenditures, the ratio of the value of the research program to its cost becomes

$$\frac{\Sigma E_n + \Sigma R_n}{\Sigma C_n + \Sigma X_{i,j,k}}$$

where $X_{i,j,k}$ represents the costs of completed projects only.

This index of the over-all risk involved in research now takes into account the performance of the organization for some time in the past. An extremely successful previous program will affect this ratio so as to indicate to management that research has a wide safety margin in which to work in the future. On the other hand, a costly and unsuccessful program will serve as a danger signal calling attention to the need for better evaluations of proposed projects or better management of research. The manner in which past performance affects this evaluation may be illustrated by an example. If the value and cost of a proposed program have been estimated at \$1,000,000 and \$200,000, respectively, the ratio of value to cost is 5. In other words, if approximately 20 per cent of the work is successful,

the program will probably break even. If to these values are added actual values and costs of recently completed projects, say, amounting to \$2,000,000 and \$300,000, respectively, the ratio becomes 6, indicative of a greater margin of safety, as follows:

$$\frac{(E_n) \$1,000,000 + (R_n) \$2,000,000}{(C_n) \$200,000 + (X_{i,j,k}) \$300,000} = 6$$

But if the value of the prior work was only \$500,000 and the cost \$300,000, the ratio of value to cost would then be 3, which would represent a greater burden to research from the standpoint of breaking even.

USE OF RESEARCH RATIOS

These indices are valuable in rationally forecasting the extent of research activity and allocating the available resources to projects which have been properly evaluated. They are also useful in appraising the results of the over-all research program and in providing a means for determining the desirability or necessity for changes in the process. For companies whose sales are subject to severe cyclical fluctuations, the combination of past with anticipated results allows the research organization to accumulate a recognizable reserve of proved values from periods of high productivity, so that a given level of desirable activity may safely be maintained.

The effect of taxes should also be taken into consideration. It should be remembered that research expenditures can be considered an operating expense for purposes of computing profits, even though they do not enter directly into the cost of production. If the enterprise does no research whatsoever, production can continue, at least for a while. We may conclude, therefore, that they are taken from the *top* of the firm's profits. If these are subject to excess profits taxes, the expenditures cost proportionately less. Where research expenditures are capitalized, the same consideration does not hold, since in this case the research investment does not directly reduce the taxable income.

The establishment of such budgets as these does not mean that the research director should be completely restricted from diverting funds from one project to another, or starting small-scale

exploratory work in connection with a new proposal. Along with the other departmental administrators in the company, he should be allowed a certain amount of latitude in altering and adding to his budget proposals. If the budget periods are lengthy, supplementary appropriations and allowances during the periods will probably be necessary. These should be added to the forecast at an appropriate time, in accordance with the established routine. An additional item in the forecast should be "miscellaneous expenditures," against which the research management would be allowed to draw resources at will, for preliminary and other work not specifically contemplated in the budget. The amount of this item will vary from organization to organization, from perhaps 1 to 10 per cent of the total budget, depending on the nature of the work. Where the greater portion of the work is exploratory, it is desirable to have this allowance larger than in the case of activity directed more pointedly toward specific process or product developments.

There are other items which may have to be allowed for in such a forecast. For example, if the manufacturing or sales organizations are permitted to request services from the research group directly, then a portion of the budget must be set aside to account for this use of resources. It may be desirable to have a separate staff and budget for this type of activity, if the size of the enterprise warrants. In any event, the proportion of the expenditures devoted to this work should be indicated so as to be clearly evident. If these requests are frequent and the individual work items are relatively minor, it will be convenient to represent their value to the company at actual cost rather than attempt to appraise the present worth of each one.

The applicability of this type of forecast and budget is as great for the small as for the large enterprise. The smaller the research expenditure, the less the difficulty and cost of obtaining the requisite information. It is much simpler, perhaps, to ignore the necessity for following these sound principles and operate in a completely uncontrolled manner. *Whatever the consequences of such a course, whether good or bad, the basic implication is that both the company's management and the research administrators are shirking the responsibility to direct their activities in a sound and rational pattern.*

TABLE VII. RESEARCH PROGRAM FORECAST AND BUDGET STATEMENT OF THE ABC ELECTRONICS COMPANY FOR THE THREE-MONTH PERIOD ENDING JAN. 1, 1951

(Net sales, 1949 = \$3,541,000; net profit, after taxes, 1949 = \$452,000)

| | Project | | | | | | | | Miscellaneous | Total |
|--|----------|-----------|----------|------------|------------|-----------|-------------|----------|---------------|-----------|
| | A | B | C | D | E | F | G | H | | |
| Estimated value..... | \$65,000 | \$160,000 | \$25,000 | \$48,000 * | \$12,000 * | \$500,000 | \$120,000 * | \$38,000 | | \$788,000 |
| Estimated time allowable for completion (3-mo. periods)..... | 8 | 15 | 4 | | | 10 | | 7 | | 44 |
| Estimated cost, this period: | | | | | | | | | | |
| Materials..... | \$600 | \$250 | \$350 | | | \$2,000 | | \$1,200 | \$1,000 | \$5,400 |
| Personnel..... | \$2,100 | \$4,100 | \$2,150 | | | \$5,000 | | \$2,100 | \$2,000 | \$17,450 |
| Total..... | \$2,700 | \$4,350 | \$2,500 | | | \$7,000 | | \$3,300 | \$3,000 | \$22,850 |
| Estimated cost, next period: | | | | | | | | | | |
| Materials..... | \$1,200 | \$700 | | | | \$1,000 | | \$500 | \$1,000 | \$4,400 |
| Personnel..... | \$3,000 | \$4,200 | | | | \$5,000 | | \$3,900 | \$2,000 | \$18,100 |
| Total..... | \$4,200 | \$4,900 | | | | \$6,000 | | \$4,400 | \$3,000 | \$22,500 |
| Estimated cost, 2d following period: | | | | | | | | | | |
| Materials..... | \$2,000 | \$1,000 | | | | \$3,000 | | \$1,200 | \$1,000 | \$8,200 |
| Personnel..... | \$3,000 | \$4,000 | | | | \$6,000 | | \$2,700 | \$2,000 | \$17,700 |
| Total..... | \$5,000 | \$5,000 | | | | \$9,000 | | \$3,900 | \$3,000 | \$25,900 |
| Estimated total cost..... | \$22,000 | \$41,000 | \$15,000 | | | \$70,000 | | \$20,000 | | \$168,000 |
| Estimated total time (3-mo. periods) to complete..... | 4 | 4 | 1 | | | 8 | | 5 | | 22 |
| Overhead costs, estimated: | | | | | | | | | | |
| This period..... | | | | | | | | | | \$15,000 |
| Next period..... | | | | | | | | | | \$15,000 |
| 2d following period..... | | | | | | | | | | \$15,000 |
| Total research costs, estimated: | | | | | | | | | | |
| This period..... | | | | | | | | | | \$37,850 |
| Next period..... | | | | | | | | | | \$37,850 |
| 2d following period..... | | | | | | | | | | \$40,900 |
| Actual expenses, to this period: | | | | | | | | | | |
| Materials..... | \$2,000 | \$6,000 | \$1,500 | \$5,500 | \$1,300 | | \$4,200 | \$400 | * | \$20,900 |
| Personnel..... | \$5,500 | \$15,000 | \$11,000 | \$12,900 | \$9,000 | | \$21,300 | \$2,300 | * | \$77,000 |
| Overhead..... | | | | | | | \$25,500 | \$2,700 | * | \$61,500 |
| Total..... | \$7,500 | \$21,000 | \$12,500 | \$18,400 | \$10,300 | | None | | * | \$158,400 |
| Actual value of project (2 years previous)..... | | | | \$40,000 | \$20,000 | | | | | \$80,000 |

* Not included in total

Table VII is illustrative of this type of forecast in an actual company, along with other data which may be of interest. It will be seen that this company is spending approximately \$130,000 per year for research, or approximately 4 per cent of net sales. The average time of completion of the five major projects in the program is $2\frac{2}{5}$ or 4.4 periods (approximately 13.2 months). The ratio of the value of this research program to its cost (including past results) is

$$\frac{\$788,000 + 60,000}{\$168,000 + 54,200 + 106,500 * } = \frac{848,000}{328,700} = 2.6$$

* Overhead.

This ratio of 2.6 is actually quite low, but it is likely that the projects, being small in number, have been carefully selected so as to have a high probability of success.

Analysis of Individual Projects

In addition to following the over-all trend and scope of the work, it is necessary that the progress of each project be evaluated and reevaluated, as we have seen. We have noted that *in each forecast* the time and cost estimated as being required for completion should be reevaluated. Therefore, with the same data, records may be kept on individual projects which will indicate their progress toward completion. An analysis such as shown in Table VIII may be set up for each one.

These progress ratios were discussed earlier and it was shown in Figure 7 above ² that a chart could be plotted in terms of the percentages of estimated time and funds expended. Such a chart graphically presents the progress of a project in the most objective terms available to the organization—estimates of the research group as to time and funds required for completion. Under such a system, bad guesses and wishful thinking will soon be apparent. We have already mentioned that an increase in estimated time and cost from budget period to budget period will be indicated by a decrease in successive ratios for a particular period, or a recession of the plotted points toward the origin. Progress is shown when the points cease to recede and the later

² Chap. VII, p. 170.

TABLE VIII. INDIVIDUAL PROJECT PROGRESS ANALYSIS (PRESENT)

| Item | Budget period | | | |
|--|---------------|---------------|---------------|----------------|
| | 1 | 2 | 3 | 4 (present) |
| Estimated value to the company..... | ${}_1E$ | ${}_2E$ | ${}_3E$ | ${}_4E$ |
| Estimated total cost..... | ${}_1C$ | ${}_2C$ | ${}_3C$ | ${}_4C$ |
| Actual cost to date..... | ${}_1X$ | ${}_2X$ | ${}_3X$ | |
| Estimated total time (periods) for completion..... | ${}_1t$ | ${}_2t$ | ${}_3t$ | ${}_4t$ |
| Progress ratios: | | | | |
| Ratio of estimated cost expended in | | | | |
| Period 1..... | ${}_1X/{}_1C$ | | | |
| Period 2..... | ${}_1X/{}_2C$ | ${}_2X/{}_2C$ | | |
| Period 3..... | ${}_1X/{}_3C$ | ${}_2X/{}_3C$ | ${}_3X/{}_3C$ | |
| Ratio of estimated time expended in | | | | |
| Period 1..... | $1/{}_1t$ | | | |
| Period 2..... | $1/{}_2t$ | ${}_2/{}_2t$ | | |
| Period 3..... | $1/{}_3t$ | ${}_2/{}_3t$ | ${}_3/{}_3t$ | |

points begin to move forward to 100 per cent expenditures and time estimates. These progress analyses will be more valuable if they are made more frequently than the budget forecasts. Thus, if the forecasts are prepared quarterly, project charts will be useful monthly; if the budget period is a year, quarterly statements of progress are desirable. Such interim statements will enable management more intelligently to arrive at or approve a research budget and forecast. A background of the progress of individual projects on the program is thus provided. Where the program is very extensive, it may not be feasible for management to review progress charts for each of the projects. In this case it may be necessary either to designate an agency of management to review the entire program and present only those projects which appear to be in some way exceptional, or to have related projects consolidated and combined charts presented. The latter alternative is not so useful or rational a procedure as the former, although it is probably better than none at all.

Accounting for Research

To derive these forecasts, budgets, and analyses from the activities of the research organization, a means must be established for the collection of the necessary data. These means will require the utilization of sound accounting methods and the development of a cost and estimate collection system which will operate efficiently without in any way hampering the work of the research group. A clear analysis of the requirements of such a system was presented by Foote and Westcott, as follows:

The results from research, however evaluated, must justify its cost. Therefore, it is advantageous both to management and to those concerned directly with the conduct of research to have informative, factual reports of research costs. This is one of the best guarantees that management can provide to insure continued support for an aggressive research program.

We are all well aware that research does not consist of routine operations conducted according to an unvarying plan and definite time-table, and that costs cannot be reported . . . on a unit basis, such as output per man-hour. Neither is it amenable to time studies nor to the ministrations of an efficiency engineer. Consequently, there is no need to assemble costs with the meticulous detail generally associated with cost accounting for operating departments of a company. *The primary objective is to provide information only in such detail as is adequate for management to determine that expenditures are within the budget, are in accordance with the approved research program, and are justified by the results obtained.* Cost data in any greater detail provide no usable information and only add unnecessarily to overhead expense chargeable against research.

A general formula for research cost accounting procedures can not be given because no two companies conduct research exactly alike. The Accounting Department . . . may be of major service to management by devising the simplest, least burdensome, and most flexible system required to satisfy the needs arrived at through a full knowledge of the company research activities.³

A basic requirement is the establishment of a focal point for the collation of the data relating to research proposals and ac-

³ P. D. Foote and B. B. Westcott, "Analyses of Research Costs," *Proceedings of the Conference on the Administration of Research*, Pennsylvania State College, *Bulletin* 29 (State College, Pa.: October, 1947). Italics added.

tivity. Depending upon the size of the organization, this focus may be one individual or a group of analysts. The qualifications of the personnel in such a group should be on the side of technical training rather than accounting, although a fundamental grasp of industrial economics is essential if the data are to be collated properly. It is in this section that the cost data collected by the accountants, the estimates and proposals for projects, the market research data on these proposals, and the other information required would be assembled into the form of budgets, forecasts, and analyses for approval by management. Such a group should be an adjunct to research administration at the management level. It would not have the function of making recommendations, but rather the task of predicting the consequences of action taken on recommendations furnished to it, in the light of all the data available. In so doing, it will be necessary that these data be critically evaluated as to accuracy, reliability, and significance. It becomes apparent that technical training and an understanding of the enterprise's production and research activities are required.

In obtaining the information necessary for management's appraisal of a research proposal, such a group, or other agency designated to furnish the required information, should set up standard methods of contacting sales, production, engineering, and the other groups which may be involved. It is not sufficient that these groups furnish the desired data only at the beginning of a project. We have seen that, for efficient operation, the project must be constantly reevaluated, and this entails a review of all the factors involved. It may be that the opinion of the various staffs and operating departments will remain the same as the work progresses, but this assumption is hardly warranted unless conditions in the enterprise are extremely static. Individual departmental forms should be provided for obtaining (1) sales potentials, (2) production costs, (3) plant investments, (4) competitive situation, (5) research costs, (6) patent situation, and (7) such other factors as may be relevant. These should be consolidated with (8) the estimates of research cost, (9) a technical appraisal of feasibility, and (10) furnished to the responsible management individual or committee for final evaluation (or reevaluation) and inclusion in the budget if approved. The same source would provide the individual progress analyses men-

tioned, calling particular attention to those which appear to be becoming static, as well as to those which are progressing ahead of estimates. *The value of such economic data to the research organization and to the enterprise cannot be overestimated.*

The accounting staff necessary to provide the actual cost data should be a part of the research group. It may, of course, be technically responsible to the general accounting department, but the particular nature of research accounts makes it desirable to provide cost-collecting personnel as a part of the group. The collection of the cost information should be such that the costs of individual projects are easily segregated, whether the work is done on a team basis or by separate sections working on individual phases. This particular segregation is somewhat simpler when project teams are utilized, since the cost of the team may be directly applied to the project, or at the most, divided among two or three projects. The over-all research expenditures must also be reported for comparison with the budgeted amount. The expenses of individual groups, whether team, subject, or separate laboratories, must also be measured and reported so as to allocate proportionate responsibility for expenditures. It is desirable also that means be provided to measure personnel, material, and overhead items for comparison with budgeted amounts. If any portion of the work is chargeable to other divisions of the company as technical service, these items must be collected and accurately transferred to those divisions. If the group, project, or laboratory administrators have no control over apportioned or prorated expense items and if these are to be included in project costs, it is desirable that they be shown separately. This is contrary to common practice in research organizations, where the professional labor charges are so fixed as to include the general administrative and other expenses. However, as noted previously, this does not make for sensitivity in reporting project costs in the research budget, nor is it likely that project or group leaders can in any way control this portion of the expenses.

In operating such an accounting system, it is necessary that the costs be reported by projects. In other words, each project will be assigned an account number, and all direct research expenses relating to that project are reported to the accounting staff under that number. Thus, direct labor may be reported weekly on an appropriate form with the days or hours worked

on particular projects indicated. Material purchases and withdrawals from stores may be reported in a similar manner. If there are overhead charges which may be directly allocated to a particular group, these may be included in project costs as a part of the direct expense. The other items of expense including utilities, service groups (except where charges may be directly related to a project), management overhead, and depreciation on the plant may be charged into an overhead account and kept separated for control purposes. Some system should be provided to place the responsibility for the expenditure of funds for purchases or unbudgeted projects with group, section, and laboratory leaders. The exact nature of the amounts that each of these is allowed to sanction is unimportant, but a uniform system is necessary and this should be in agreement with general company fiduciary policy. The account numbers should be so designed as to provide a simple method of (1) determining the division from which the charges are made [and in the case of intergroup activity, (2) the division for which the work was done], (3) the project, as well as (4) the applicable charges to a particular operating department. It may also be desirable to indicate (5) the general classification of the project, if the organization is large. These account numbers may serve as file classifications throughout the company, since this will provide a uniform grouping system and allow the compilation of a complete project file, including correspondence, reports, and costs without unnecessary duplication and cross-referencing.

Costs of Various Types of Research Organizations

The relative costs of the various types of research organizations discussed in the previous chapter have been subject to much debate. It has been concluded, although admittedly with little confirmatory data, that the subject-type organization is the least expensive to operate.⁴ The problem-team organization

⁴ C. E. K. Mees, *The Path of Science* (New York: John Wiley & Sons, Inc., 1946), p. 223. ". . . it is probable that the project system materially increases the cost of operating a research laboratory and does not produce an equivalent efficiency in results." Mees assumes that under the project system the group leader will be a businessman with scientific training, and that the work will be directed from above. As we have seen, there is no

is considered the most costly. It is our opinion that *the efficiency of research is to be adjudged by the nature of the results obtained*, and not by the overhead or expenditure per creative worker. (This does not imply that such considerations are unimportant.) *Therefore, with proper administration and organization, the least costly research organization is that which produces the most efficient results for the least expenditure.* Based upon our previous analyses, it is likely that the problem-team pattern will best achieve this objective. The use of a system based on these requirements will provide all the data necessary to maintain adequate and comprehensive research budgetary control.⁵ By comparing these values with estimates of cost and time made by the research department itself, and with valuations placed on anticipated results by management, the handling of research funds will be placed on a sound and efficient basis. Research will then not be regarded as an esoteric activity by nontechnical management, to be engaged in with fond hopes and expectations when sales and profits are high, and to be curtailed, reduced, or cursed under other conditions. It can take its place along with the other departments of an enterprise as a rational and necessary activity, to be directed, managed, and controlled in an efficient and objective manner, and thus to provide its share of the company's profits.

a priori reason for reaching this conclusion. The subject-type laboratory can be, and has been, as badly misdirected as the project-type organization.

⁵ See also W. Rautenstrauch and R. Villers, *Budgetary Control* (New York: Funk & Wagnalls Company, 1950), Chap. XIII.

CHAPTER X

INTERNAL RELATIONSHIPS IN THE RESEARCH GROUP

The researcher, whether in administration or at the laboratory bench, should be delegated the requisite authority to direct and control all types of resources necessary for solving the problems to which he is assigned. Further, it is important that this delegation proceed from such a source that management's policy will have the greatest possibility of being carried out. There are various methods of systematizing the internal relationships in a research organization to achieve these desired objectives. Some of the basic over-all patterns were discussed in Chapter VIII. If the general principles already outlined are to be specifically usable, it is necessary to define in greater detail the functions and interdependence of the personnel within the group.

No fixed organization chart should be expected to be suitable in more than a few cases. We shall, therefore, outline the nature of specific functions in the light of our previous discussions, and then examine certain actual organizations to determine the manner in which they are carried out. The research workers and administrators will be considered from the standpoint of general duties, manner of accomplishment of these duties, status, relations with assistants and superiors, and means of reporting achievement. We shall begin with the heart of the research organization—the creative personnel at the so-called laboratory-bench level—and then consider in turn the immediate supervisor of such personnel (as exemplified by group and project leaders), administrative directors, service workers and administrators, and top management directors. These are the general functional categories which may be correlated with any existing research establishment, *i.e.*, the “building blocks” which can be utilized to design a new or improved organization. No matter what pattern of operation, such as subject, functional, or project, is con-

sidered desirable, the basic relationships which we shall discuss will be found to hold.

The Research Worker

The duties of the research worker have already been defined as being the solution of problems. We have pointed out that the solution of problems involves (1) the observation and collection of data, (2) the manipulation of these data so as to hypothecate a solution or solutions, and (3) the verification and extrapolation of these hypotheses in instances of specific interest. This general classification may perhaps be extended to any category in the research organization, and it is now necessary to determine the characteristics of the work which distinguish the personnel at the bench level from the other classes which will be considered. The key difference lies in the *emphasis* placed upon "observation" and "manipulation" at this level. From a practical standpoint there is a great deal of noncreative work which must be done in any research organization but which requires formal technical training. This work consists largely in the collection of data or the manipulation of such data in some more or less predetermined manner. It is a responsibility of this class of personnel to carry out these operations. Such a distinction holds in any of the areas of industrial research which have been described—market, process, product, organizational, etc. It certainly does not preclude the researchers in this classification from engaging in creative activity, which they can and should do. It is only a recognition of the fact that observation and manipulation of data are essential parts of the process, and that in a great many instances *there is a functional level above which such activity is not undertaken. The research worker, in this sense, is then the technical employee whose activities are primarily concerned with observations and manipulation.* His function is still that of problem solution but, in contradistinction to higher levels, he is not concerned with the coordination of other problem-solving units into a collective whole. However, he still may have assistants and be concerned with some administrative duties, as we shall see.

The manner in which the worker accomplishes his duties in this regard will vary, depending upon the type of work under-

taken. The researcher in applied mathematics or market analysis groups will not follow the same pattern of activity as the chemist or physicist in an organic synthesis laboratory. The general operational characteristics necessary for efficient work will be common to all of these. The worker will find, or will be given, a specific task to accomplish. He may be given either the method or the data necessary for its fulfillment; if he is given both, the job is no longer one of research and may be undertaken by a skilled technician. He may, or may not, be directed to arrive at the required results in a specified manner, or the general direction which his work will take may be determined by collective agreement. In any case, he may undertake any of the following activities: (1) the direction of the design of necessary equipment; (2) the determination of the observations which will be required, by design of experiments, questionnaires, etc.; (3) the actual conduct of experiments and collection, or direction of the collection, of data; (4) studies of the literature; (5) the manipulations and calculations involved in correlating and deciding upon the significance and relevance of data; (6) the reaching of conclusions; and (7) the presentation of results.

It is not expected that he would necessarily be able to work alone in carrying out the above activities. On the contrary, it is quite likely that he will have assistants and collaborators, who may range in number from one to a score or more. Some of these aides will be researchers in training, others will be technicians who will perform the routine duties of reading instruments, manipulating apparatus in a predetermined manner, collating observations, and such other duties which might not require the more mature judgment and experience of the researcher himself. In considering the activities of this research group or cell, it should be recalled that this unit could be working upon a single problem, or upon a phase of a larger problem, and is not concerned with the coordination of more than one phase or problem. It is the basic unit in any research organization, and with it we may design a collective group of any desired complexity. While the researcher here may not be directly concerned with coordination, nevertheless, much of his activity may require active cooperation with similar units in the organization.

In view of these activities, we may picture the status of the research worker as indicated by Figure 8. The solid lines are intended to indicate channels of communication, and the dotted lines areas of his authority. Clearly, not all his channels of communication can be depicted, nor do these indications of his authority define that authority in a given case. No implications of responsibility have been indicated, since this in large measure

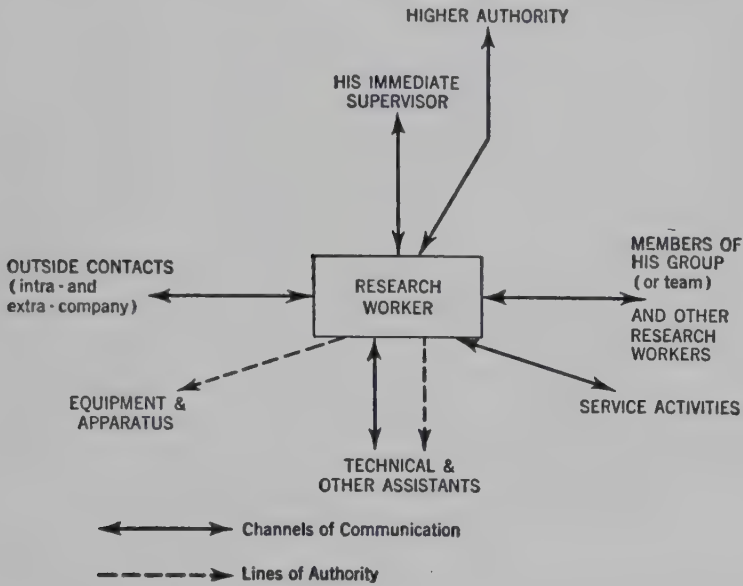


FIG. 8. The research worker's organizational relationships.

depends upon the manner of its delegation. However, we have defined that obligation as being one of problem solution and may assume that it extends through various intermediaries to management. It ordinarily should be owed to his immediate supervisor for proper organizational efficiency, but this is not always the case in research groups.

In examining Figure 8, it will be seen that the authority of the research worker extends to his assistants whom he directs in the performance of the activities described previously, and to the equipment or apparatus for whose manipulation he is responsible. In a strict sense, of course, he cannot direct such equipment, but authority generally implies the power to administer, in which case the line is correct. In some organizations the research worker may be given authority over service groups, but

generally his relationship with such personnel is that of a requester of their services.

His channels of communication are often quite extensive and are certainly most important. As has been emphasized, *the more sources of information made available to him, the more likely he is to bring a broad attitude toward the solution of his problems.* The most important of these channels, from an administrative standpoint, are those between himself and his immediate supervisor, and between himself and his assistants. His supervisor will establish the extent of his understanding of the problems he is given to solve and will act as a catalytic agent in coordinating and orienting the worker's activities along those lines which will be most profitable to the enterprise. Where relationships with other groups and activities are involved, his superior will be most important in opening new channels of communication and seeing to it that the worker is supplied with the required information and materials. As far as his assistants are concerned, he must be given authority over them so that he can carry out the activities for which he is responsible. Experience has shown it to be most inefficient for assistants to work *with but not for* a researcher.¹ His communication with the assistants must be direct and effective. It is clearly advisable to establish a research cell such as this in a single physical location, and this is commonly done in industrial organizations. Thus, most laboratories are constructed so that the worker and his assistants are given desk space in the room in which they carry out their work. Where this is not expedient, they share the same office. *Communication at this level should be direct and oral.* Where the extent of the activities and the number of assistants are so great that this is not possible, then the cell has probably grown too large and should be broken up into smaller units.

Means should be provided for communication between the worker and his fellow researchers, who are not necessarily working on the same problem but who occupy similar positions in the organization. Such contacts may be most important in ensuring a wide dissemination of available information and attitudes,

¹ This does not extend to group activity in which the workers may be on an equal footing.

which is so necessary for successful creative work. For this reason, any type of organizational or compensation patterns which would tend to engender secrecy or jealousy would be extremely inefficient. Such communication should be on both formal and informal levels; *i.e.*, an opportunity must be provided for the research workers to gather and discuss their problems. They must also be given access to, and the time to study, the reports and memoranda of their fellow workers concerning the remainder of the research activity of the enterprise. This communication will naturally be much more direct and extensive with members of their own group or team than with workers in removed and unrelated activities.

In all cases the researcher will have communication with professionals and technologists outside of the company. We have seen that it is not only worth while but essential for the enterprise to encourage such contacts. Where this has been done on an extensive scale, it has proved beneficial for the morale and personal satisfactions of the individual, for his professional growth, and directly for the research problems of the organization.

In addition to these extra-company connections, it is often advisable to recognize that contacts with personnel of other divisions of the company are useful and, in some cases, necessary. An acquaintance with production, sales, and other problems (which can generally only be obtained by direct contact) may be invaluable in increasing the productivity and hence the efficiency of research. We have shown in Figure 8 a channel of communication with higher authority which does not extend *through* the worker's immediate supervisor. It is to be anticipated that the administrators and directors in the research group will take a direct interest in the various activities of the organization. It is also to be expected that they will have direct contact with the individuals engaged in this work. Such contact will be on the technical level, and the communication involved may modify or influence the work being carried on. None of this in any way changes the authority or responsibility of the researcher's immediate superior, so long as no changes or modifications are made in any policy decisions communicated to the worker by his supervisor.

It has been suggested that the worker's communication with his supervisor must be facile and direct. *It is generally true that the more formal the report on research activities, the longer it will take to influence and modify the over-all picture.* The most informal report the research worker can make is an oral one, and the communication time lag rapidly increases as he utilizes the more rigorous forms of reporting, from the memorandum to the research report. It is not intended to imply that all communications should be oral or informal—far from it—but it is necessary to emphasize that this type of reporting occupies an important place in the organizational structure. The following chapter will discuss in detail the various types of research reports which may be used. However, in so far as the relationships of the researcher vis-à-vis his immediate supervisor are concerned, his reporting of his achievements will influence the general aspects of a research program to a large extent. Occasionally it may be that only one such worker is responsible for the solution of a major problem, and in this case the coordination of a group or team by a supervisor depends directly on such reports. Since the supervisor probably takes action in advance of any formal report, it should be clear that the number of workers making these informal reports to a single individual is of necessity determined by sheer physical limitations.

This cell or unit of activity, consisting of the research worker and his assistants with such channels of communication and lines of authority as we have described, may be placed in any type of general organizational setup. Unless it is a most unusual one, the relationships we have noted generally remain stable, and the pattern of organization makes little difference to his function and modes of activity. This is true, since by definition, he should be given the necessary authority to carry out his duties. *It is important to repeat that this does not mean that the work will be carried out with equivalent efficiency in any of the possible organizational models.* From the standpoint of ease of communication through the various channels mentioned, the team, or project approach, which has been discussed as having merit as far as efficiency is concerned, offers the greatest opportunity to maintain the broadest possible contacts. This is true because

1. The project-team supervisor will be directing and coordinating all the members of his group for the solution of a specific problem.
2. The various members of the group, who would ordinarily be organizationally separated by functional divisions, will have direct contact with those working on the same project.
3. The individual researcher will be able to obtain quickly and efficiently a collective view of his specific problems.
4. The project supervisor will be in a position rapidly to change orientation of the group members in accordance with information obtained.

The Research Group Supervisor

The research worker must report to some individual if the organization is to be effective. We have called this person his "immediate supervisor." In the small organization, this individual may be director of the research organization; in the larger group, he may be the team, project, or functional supervisor. In any case, if the workers already discussed report directly to him, he fills the organizational role at this level of activity. The emphasis in this instance is upon his duties of coordinating more than one worker in a more or less homogeneous group. He is ordinarily delegated the larger responsibility for resolving more than one problem, or for the over-all aspects of a major objective. Although he is not barred from direct laboratory or field work, he will nonetheless not have the time or opportunity for continued observation and "manipulation" in any single phase of the activity which he supervises. Again, it is expected and intended that he shall engage in creative activity, but the routine duties of coordination and administration must also be accomplished.

His duties comprise (1) the general orientation of the course of the observational and manipulatory work being undertaken by the workers he supervises; (2) the review and approval of the data, methods, results, and conclusions which have been reached; (3) the application of his intelligence to the problem (or problems) and the results attained to assist his workers in arriving at solutions; (4) the maintenance of efficient and technologically satisfactory methods of work in his group; (5) attention

to morale and personnel requirements; and (6) presentation of results to higher authority, along with (7) recommendations for further activity. If the research workers make up the heart of the organization, certainly their immediate supervisors are the nerve centers. *A more exacting combination of both creative and administrative ability is called for at this supervisory level than in any other place in the research organizational structure.*

In a sense, the manner in which the supervisor carries out his duties does not vary so much from one type of work to another as for the individual worker. It will be his responsibility to direct several workers in accomplishing their assigned tasks. He will have to determine (in collaboration with the workers) whether the data and methods are useful, significant, and relevant. He will assist in all the modes of activity of the workers under him. Since his time is necessarily divided among more than one such activity (by definition), his assistance should be directly utilized on the bench level at only the critical junctures. He is given the responsibility for solving the problems assigned to his group and will, therefore, be directly concerned with the design of equipment, apparatus, experiments, questionnaires, and methodology, as well as reaching conclusions and presenting them for further action. The number of workers whom he will supervise may range from one to perhaps ten. In the Armour Research Foundation, for example, the number of workers reporting to such a supervisor averages approximately five.²

This combination of the units or cells reporting to a supervisor forms a major phase of the structure; it may be the entire organization, or the major collective component of a larger activity. The supervisor will ordinarily be expected to carry out such administrative duties as the preparation or approval of requests for funds, equipment, and services, reports of expenditures, maintenance of inventory reports, and so forth. If his group is large, he may be assigned clerical and administrative assistance to carry out these activities so that he may devote the greater part of his time to technical work. It is important that this function does not evolve into a strictly "paper-shuffling activity," as

² It should be recalled that these workers will have their own assistants. Thus, at the Armour Research Foundation there are two research workers to every assistant, and the average group would consist of a supervisor, five workers, and perhaps three technical assistants.

has happened in a number of "well-organized" research groups. The major activity of this supervisor should be coordinative and stimulative, and this requires both a high degree of technical competence and administrative ability. *If the workers whose activities he is superintending find him technically dull and unimaginative, it is not likely that he will be able to inspire them to high levels of creative work.*

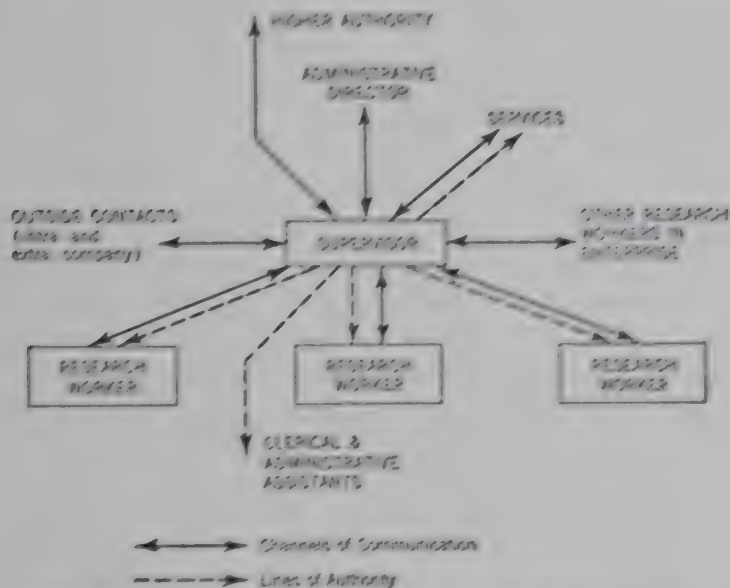


FIG. 9. The research worker's immediate supervisor.

In a manner similar to the previous chart indicating the status of the research worker, Figure 9 depicts the supervisor as a unit in the organization. Channels of communication and the lines of his authority are shown. Since he is given the responsibility for the solution of problems in his group, his authority must extend to the research workers as indicated. The nature of his personality and his (or the organization's) method of operation with respect to this authority should not change or modify it or the responsibility obligation which makes the authority delegation necessary. On the other hand, these factors will certainly influence the efficiency of the process, as has been discussed.

The supervisor may also have authority over various administrative and clerical workers assisting him and his group, and perhaps over some services. The latter might be particularly

true in some types of pilot plant operation where steam fitters, say, were continuously needed and were made part of the project activity under the direct control of the supervisor. However, the more usual case is one in which such services are available upon request and are under separate and specialized authority.

In achieving his objectives, it should be apparent that *the supervisor's most important channels of communication are with the workers under him*. This is clearly a reciprocal relationship, and we have already emphasized the necessity for stimulating contact between worker and supervisor. It is necessary that he present the problems to be undertaken by his group in the most understandable manner possible. He will have the closest contact of all research management with the working level and, therefore, must be prepared to evaluate and judge the abilities of the personnel under him. When a problem is advanced, it is the supervisor who is in the best position to determine whether it is within the capabilities of the members of his group to solve and, as we have seen, to ask a researcher to attempt to solve a problem which is beyond his ability is most inefficient. The operation of such a unit on a democratic and collective basis requires that he have a personality suitable for the maintenance of clear and open channels of communication with those under him. In other words, he must be able to approach others easily (and be approached by them), he must not be indifferent to the actions of people, and he must not show antagonism in his dealings with them.

It is not likely that such a group under one supervisor can be placed in immediate physical contiguity but, where possible, facility for direct contact among group members and between them and the supervisor is most desirable. As was noted above, the informal report of the worker to the supervisor is a most important form of communication in coordinating and directing the efforts of a group or team. This informal communication should not be hasty and should not be difficult to achieve, or else the information passed on may be distorted. Distortion of information at this or any other point, whether deliberate or accidental, can lead only to inefficiencies in the research process. Therefore, the supervisor must devote a great deal of his time to individual or group conferences with his workers. It is advisable to consider having a daily conference with each worker,

and perhaps a weekly or semiweekly conference with the entire group, on a routine basis. The establishment of recognized channels of communication in any organization is a most important factor leading to successful administration. Where possible, it is desirable to have concise reports made of these routine conferences, in writing, for all the participants. It has been found that such reports are well worth the effort required for their preparation, in that they increase the efficiency of information dissemination and reduce errors of understanding arising from misinterpretation. In reviewing the more formal reports which are distributed to other members of the enterprise, the supervisor must have a clear picture of the work that his group has done. Otherwise, he will be in no position to add to, or revise, the conclusions which may have been reached. No matter how clear and complete the reports may be, he will not be able to do this unless he has had a complete understanding of the work as it has proceeded.

The supervisor must also have a clear picture of the actual research problems, and this he must obtain from his superior. It is in this relationship that the limits are placed upon the activities of the working group. The supervisor must report over-all progress of his group to the next higher level, both orally and in writing. From an informal standpoint, it is not necessary that this communication be as frequent or detailed as those with his own workers. Since he should have the authority to make a majority of the decisions on internal group matters without reference to higher authority, an extremely close relationship is not required. Where matters of policy are concerned and where there are major difficulties and delays in the research program, access to this higher authority *must* be available. As far as formal reporting is concerned, the supervisor will ordinarily be passing on the reports of the workers of his group. However, where a coordinated report is desirable, he will have to direct the collaborative efforts involved in its preparation, and perhaps write it himself.

It is highly important that the group supervisor establish and maintain contacts with the other research activities as well as with those divisions of the company to which his work is related. For example, a market research group supervisor should have a complete understanding of the sales situation and of the

distribution facilities of the enterprise. Similarly, a product research group leader should be in close communication with the production facilities and market research divisions. A research group does not function in an atmosphere of complete detachment from the rest of the company's activities, and the leader of the group must be provided with the opportunity to communicate with the remainder of the organization.

His communications with higher authority, not passing through his immediate superior, are similar to those described for the individual worker. Again, such contact implies no by-passing of authority but is a necessary and inevitable adjunct to the type of operations such personnel are engaged in. His contacts with outside professionals are also important for the reasons previously described. It is likely that these will be even more valuable to the supervisor than to the individual workers, since he will be involved in directing and coordinating a technical program of wider scope than any of the workers under him.

This administrative unit, consisting of the group supervisor, the research workers, and their assistants, will, of course, be affected in composition by the type of organization in which it is placed. In a subject organization, such a group might consist of organic chemists, or nuclear physicists, etc., while in a functional pattern, it might be a product research division, with suitable personnel. Nevertheless, the administrative relationships are not changed, and this unit may be considered as the basic *complete* administrative group in the research process.

The Administrative Director

If the research organization consists of more than one supervisory unit, it will require an administrative director to whom the group supervisors report and through whom they receive their authority. In the small organization where there might be only one such group, we may already have reached the top management level. For the purposes of this discussion, we shall assume that the organization we are discussing is sufficiently large so that some administrative direction must be interposed between the groups and management. It should be apparent that the larger organizations may require additional administrative personnel above the level of the administrative director, since

the span of control of an individual is limited. In any event, the functional relationships which we shall discuss are quite similar, and it is not necessary to carry the analysis of this type of direction further.

At this stage, technical brilliance, although quite possibly useful, assumes a definitely secondary role to administrative and managerial ability. *While the administrative director may be able to establish a pace for a high level of activity and to inspire his group leaders and their subordinates to productive achievements, he will not ordinarily be able to participate in the actual work itself.* This is the distinguishing characteristic of this functional level. His duties are generally to supervise, coordinate, and control the technical and nontechnical activities of the research groups and the service activities reporting to him, as well as to control the personnel and organizational functions of these groups.³ His primary responsibility is for the *efficient* attainment of the research objectives delegated to his jurisdiction.

To this end, he should carry out his obligations by making sure that the proper resources are provided to the groups for the solution of the problems assigned to them. Such resources will include both personnel and equipment. It will also be necessary for him to establish the basic procedures and regulations under which the groups will operate so that management's policies can be effectively carried out. He will probably issue the formal instructions for the undertaking of research projects and make the *final*, formal reporting of achievement and progress on these projects to management. Also, his assistance will be required in the formulation of programs and budgets. He will usually review for management the technical feasibility of the proposed work as well as present estimates of the probable costs.

The number of groups, including service activities, reporting to such an administrative director can range from one to ten, averaging approximately five, in most organizations. He may also need a number of technical and administrative assistants in order to be able to carry out his obligations satisfactorily.

³ If there are additional administrative functions above this level, some of the responsibilities described may be reserved for them. This would be the case if there were more than one functional administrative director reporting to a research director. However, the general principles discussed are not changed by such an arrangement.

The technical assistants may serve in the capacity of advisors on various phases of the research program, furnishing him with general over-all guidance for the formulation and review of objectives and progress. Administrative assistants may be necessary to assist him with budgetary, control, and personnel policies. In general, such assistants are not given administrative authority but partake of the director's responsibility for the coordination of the program. As far as the research organiza-

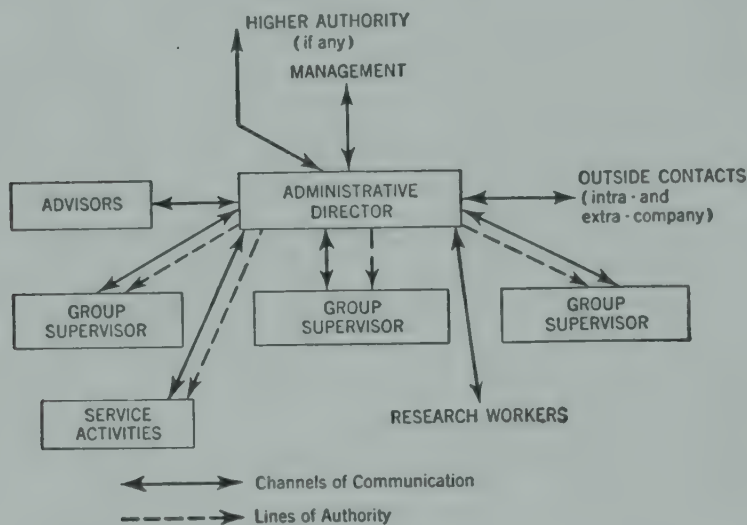


FIG. 10. The administrative director.

tion is concerned, any directives resulting from their advice should be issued by the director in accordance with the lines of his authority.

Figure 10 shows the relationships of communication and authority surrounding the administrative director. His authority extends to the group supervisors, to his advisors, and to the service activities including craft divisions, accounting, analytical, library, and similar groupings where they are placed under his control. All of this authority is necessary if he has the obligation to achieve the research objectives of the enterprise and at the same time maintain his expenditures within budgetary limitations. As has been noted before, he cannot *direct* his personnel to be creative, but he must so select and coordinate the activities of his group supervisors that the desired ends will be attained. His responsibilities for advising management of the technical feasibility of proposed research work have been outlined. There-

fore, he will have to understand thoroughly the abilities and knowledge of his organization and should not be required to undertake research which he considers beyond its capacity. He will have to direct the service activities in such a manner that the various groups are furnished with their requirements equitably and efficiently.

In the case of the administrative director, all the channels of communication shown in Figure 10 are of the utmost importance. His relations with the group supervisors should be such that he understands the technical and administrative situation in each of the groups. (In the larger organization it may be necessary for his assistants to obtain this information for him.) In most cases, the informal reports on progress that he can receive may be so brief as to be misleading and, therefore, he will be forced to rely upon the formal reports of the work accomplished. It is too much to expect that time will be available in the organization for a daily meeting of the group supervisors with the administrative director, but where possible such conferences should be established on a weekly basis. The situation is similar to that outlined for the group supervisors, and concise reports on these meetings should be available for reference purposes. If the director is to rely on his advisers for satisfactory operational advice, he must place a great deal of trust and confidence in their judgment. Since their duties will not ordinarily be administrative, he should expect creative thinking from them with regard to the technical problems of his program. These advisers should be the best of the creative personnel in the organization and usually do not require the added attributes of administrative and managerial ability.

The director must be given complete control over the employees under him, both for salary and wage status and changes, possibly within previously approved brackets, and for reductions in force and changes in position. He should also be given formal authority to expend limited amounts of money not previously included in a budget or formal program, and to initiate exploratory projects without reference to higher authority within these limits. He should assign to his group supervisors priorities for their work in accordance with basic management policy. He will probably effect such actions through his direct channel of communication with his group supervisors.

His contacts with management and with the other divisional operating personnel within the company must be sufficiently broad and complete so as to ensure the maintenance of a sound understanding of his research activity throughout the organization. It is he who will present the over-all research viewpoint, so he should, as a part of his direct responsibility, make distinct efforts to maintain clear and efficient channels of communication with this personnel. He must keep himself aware of the problems of production, sales, purchasing, etc., so that the research organization can reflect general company activities in its work. He must present to management measures of his achievement and progress, as well as interpret the meaning of the results obtained.

It should be clear that *the administrative director's position is one which requires the highest degree of administrative and managerial competence, as well as an ability to comprehend the technical problems faced by the research staff.* It is not essential that he contribute to the creative activity, but he must be able to surround himself with men who can do so. As we have noted in the larger organization, there may be several such administrative directors who report to a director on a still higher level. In this case, those on the lower level would probably be divided by some functional arrangement into the type of organizations that we have been describing. At the same time, the relations with management would be reserved for the superior director, as would some of the other administrative functions, including perhaps the authority for initiation of projects, salary and wage policy, etc. However, from this point upward, functions are not added but merely divided for purposes of satisfactory administrative control. The combination of a number of groups reporting to an administrative director can form a large-scale complete research operation. From the organizational standpoint, so long as the responsibilities of each level and the lines of authority are kept clear, the types of divisions utilized may be of little importance.

In order that the director may maintain proper control over the service activities within his organization, it may be desirable to have these groups report to a single manager of services, who in turn will report to the director. This position may be

coordinative with the other group supervisors or, if the organization is a large one, coordinative with the administrative directors who report to the top-level director. From a point of view of efficient operation, such a group should include all those services which will be required by the operating research workers and their administrators on a routine basis. Therefore, such functions as the library, data divisions, maintenance and crafts, drafting activities, or analytical laboratories may be included, the exact nature of the various activities depending upon the objectives and program of the research organization. Communication between the service groups and *each one* of the functional units is essential. Since the authority over the service groups extends from above, it is necessary that the director provide *usable* channels of communication in each of these cases. The service functions should be used strictly for the purpose of reinforcing the creative abilities of the research workers. *If it is not possible to obtain needed service easily and without red tape, then the research unit cannot operate at a high level of efficiency.* It is for this reason that self-containment of the organization, including all necessary services, is recommended. Supervisors of service groups and the research administrators responsible for their control should keep in mind that these groups can, of themselves, produce no solutions to research problems.

Management Direction

The authority to carry out the research program must be delegated to the director by management, to whom he in turn must report. The responsibilities of management for the research program and its direction were discussed at length in Chapters VI and VII. It was shown that management must accept the ultimate responsibility for the choice of a research program and should delegate to its research administrator the necessary authority to enable him to solve the problems which exist within this program. We have further indicated that the extent of the authority that may be delegated and, therefore, the limits of the problems that can satisfactorily be undertaken, depend upon its source. The official from whom this authority emanates may be a plant manager, a vice-president in charge of research, a vice-

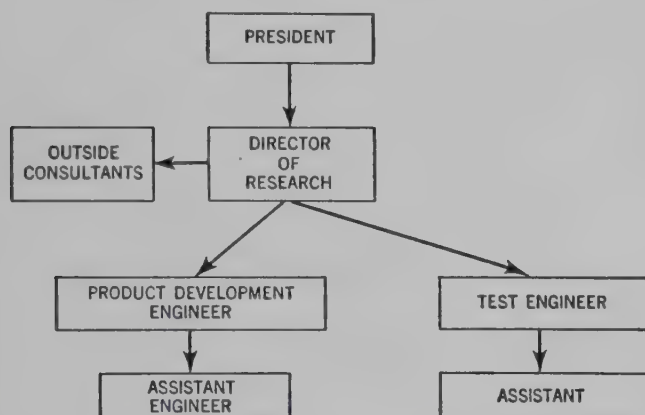
president in charge of an operating division, or the general manager of the enterprise. In any case, the area of communications of this official with the remainder of the organization should be sufficiently complete to enable the research program to be chosen in the light of the criteria described. It is not essential that he be technically trained, but the research director must be prepared to make clear to him the implications of the research undertaken. He in turn, through the board of directors, executive committee, research committee, or other communicating and control agency, must obtain the necessary approval and support of the research program.⁴ It may be desirable for the top-management research representative to have a permanent staff to evaluate the proposed and going programs and to give him an impartial analysis of progress and achievement. Such a staff would be solely advisory to him and would collate and coordinate all the available information concerning the present and future research program. It would deal largely with the area of research economics and should include personnel with a broad background in technology, as well as an understanding of industrial economics.

In established research organizations these functional patterns are not difficult to discern. Each one will have been set up, either deliberately or through evolution, to attain more or less definite objectives. In chart form they may appear atypical, but the basic relationships which we have outlined should clearly exist. The examples selected for examination range in size from the very small research unit to the extremely large organization. The charts include only the lines of authority and have been deliberately simplified to make the relationships explicit. The channels of communication which have been described exist, of course, but their inclusion in these charts would have made them unnecessarily complex.

⁴ L. W. Bass, "Management of the Well Developed Research Program," *Chemical Engineering*, Vol. 53 (July, 1946), p. 127. "It is widely recognized that the research director should report to a general executive of the company, or should himself be a company officer. By this means, the program is given a status that protects it from the encroachments of sales service, or production trouble shooting, which can easily become restrictive."

Research in a Small Company

The organization shown in Figure 11 is that of a small metal-working company whose gross sales were approximately 1 million dollars at the time of this analysis. A research group was established to develop products for manufacture and sale which would provide a complementary source of revenue for anticipated



Cost of Operation (1948)

| | |
|------------------|----------|
| Director..... | \$10,000 |
| Staff..... | 16,000 |
| Consultants..... | 3,000 |
| Miscellaneous... | 3,000 |
| Overhead..... | 1,500 |
| Total..... | \$33,500 |

FIG. 11. Research organization of a small metalworking business (gross sales, approximately 1 million dollars per year).

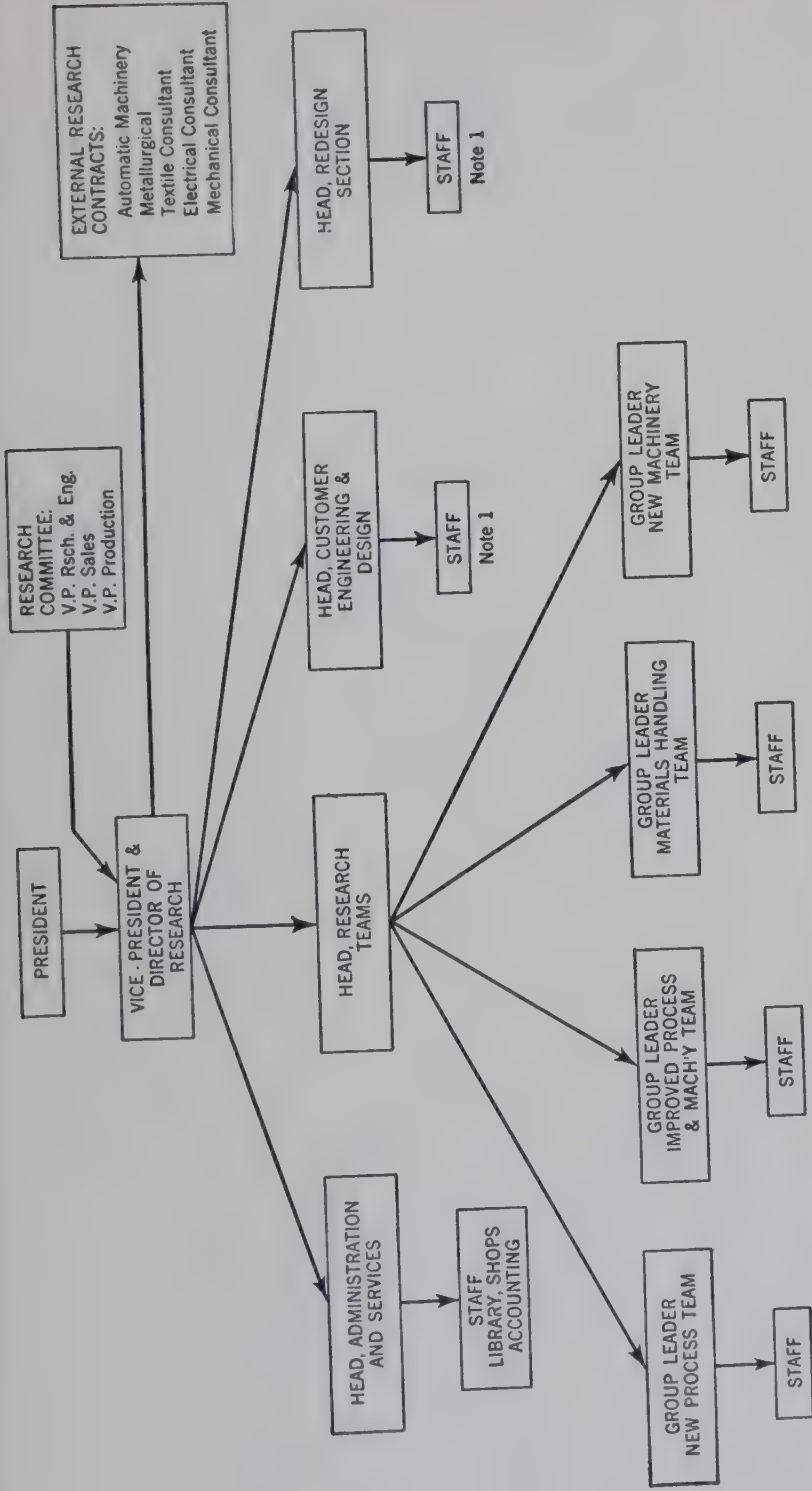
cyclical fluctuations in the company's primary business of contract work. The group necessarily had to be small, and it also had to have well-defined concrete objectives if it were not to constitute a severe drain on the working capital of the enterprise. It did have additional utility in ensuring that required quality considerations of the contractual work could be and were met.

It will be seen that the organization comprises an administrative director reporting directly to top management and controlling the operations of two group leaders. The director also coordinates the operations of such work as may be outside the

scope of the group and for which outside organizations are utilized. The cost of operating this activity was approximately \$33,500 in 1948, broken down as shown in Figure 11. Such an organization as this, with its authority emanating from the senior officer and with channels of communication unobstructed by complex administrative relationships, can be extremely effective in the small company. A necessary proviso is, of course, the choice of objectives within the capabilities of the group to accomplish. During 1948, this research organization is estimated to have saved the company some \$25,000 in quality control of contract machine work. A new product developed by this department and placed on the market in the latter part of 1948 is estimated to have produced a net profit of approximately \$22,000 by July 1 of 1949.

Research in a Medium-sized Company

A larger and more complex organization is illustrated in Figure 12. This is the research and design engineering department of a well-established manufacturer of textile machinery whose gross sales are approximately \$15,000,000 annually. The research groups evolved largely as adjuncts of the engineering design department and were (at the time of this analysis) under the same direction. The expense of customer engineering and design and of redesign of existing machinery is charged directly as part of the costs of production and is not included in research expenditures. Therefore, except for administration, these groups need not be considered a part of the research organization. The intermediate level of research administration is handled by the heads of the research teams, each of which is under a group leader. These teams are functionally divided and may have one or several objectives. For example, the New Process Team has the single project of developing a process for the manufacture of nonwoven cloth, while the Improved Process and Machinery Team has numerous projects for the improvement of the standard products of the company. It has been the policy of this company to establish a number of projects with external consulting or research firms. The responsibility for communication and coordination with these organizations rests with the research director. The head of the service group is also under his direct



Note 1. These groups are charged directly to customer and not included in research costs.

FIG. 12. Research organization of a manufacturer of textile machinery (gross sales, approximately 15 million dollars). Research cost, 1948, approximately \$325,000.

supervision. The director of research is a vice-president, and as such, reports to the president of the corporation. Thus, we have built up from the research worker, through his group leaders, and intermediate administrative direction to the vice-president and director of research. The expenditures of this organization, not including customer engineering and design, or redesign, were approximately \$325,000 in 1948.

Research and Quality Control in a Large Company

Figure 13 illustrates the organization of a research group in a company in which quality control is a paramount consideration. The company in which it is established is a specialty chemical products manufacturer whose gross sales are approximately \$50,000,000 annually. There are three major administrative divisions in the research department, product engineering, research, and services, each reporting through an administrative director to the director of research, who in turn reports to the vice-president of manufacturing and research. Quality of product is of such great importance that the director of the product engineering laboratories is responsible not only for quality control but for placing new products into production and technical service. The process development section establishes a new process in the manufacturing plant, and *in collaboration with* the chemical and physical laboratories directs the standardization section in establishing the requisite standards of operation. The quality control section maintains the necessary measurements of quality control limits over the product. The technical service section works with the customers and returns information to both the standardization and the quality control sections for changes as required. The lines of authority in this division of the organization are not clearly drawn, and it is likely that what are intended to be such lines are more likely channels of communication. However, to all intents and purposes, the standardization, quality control, and technical service sections are subgroups on a slightly lower status level than the others. In a sense, this confusion of authority would make the director of the product engineering laboratories more directly responsible for their performance without an intervening administrative level.

The exploratory and transitional research is done in the func-

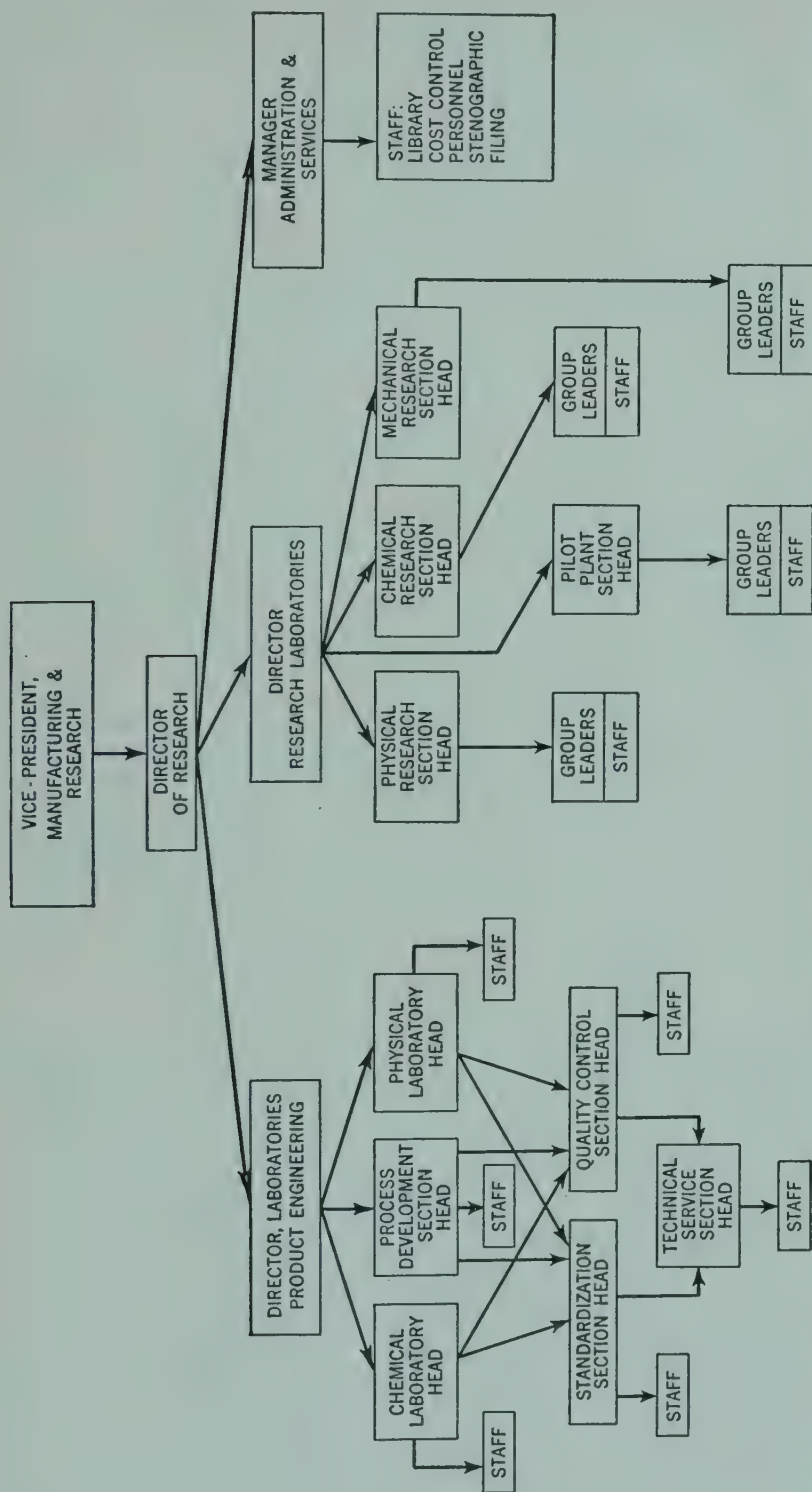


Fig. 13. Research organization of a manufacturer of chemical products (gross sales, approximately 50 million dollars). Research cost, 1948, approximately \$645,000.

tional groups under the director of the research laboratories, not to be confused with the director of research. There are four groups here, physical research, chemical research, mechanical research, and pilot plant, each under an administrative director, or section head, to whom group leaders report. This division is more clean-cut organizationally and may be able to carry out its duties more efficiently for that reason.

All service activities are administered by a section manager, who reports to the director of research. It will be seen that, as organizations increase in size and their objectives become more complex, the number of levels of administrative direction is considerably increased. It should also be apparent that the particular problems of each enterprise will in large measure designate the specific arrangements of the functional personnel. In the case at hand, the fact that research centers almost entirely about the manufacturing problems of quality and standards colors the organization from the highest level to the laboratory bench. The top-management authority is delegated downward from the vice-president in charge of manufacturing and research. A major division of the organization has an extremely complex arrangement of personnel devoted to problems of standards and quality. The cost of operating this setup in 1948 was approximately \$645,000.

Research in a Large Oil Company

The last organization we shall discuss is that of a major oil company whose annual sales are in excess of 500 million dollars. This pattern, which is illustrated in Figure 14, has been considerably simplified in the interests of clarity. It is perhaps typical of the way the larger organizations are built up in functional divisions of the fundamental cells previously described. The top-management representative of research is the vice-president in charge of research, to whom the administrative director reports. Under this director, there are several functional divisions representing the major interests of the corporation. Each of these divisions and the business administration and control function are administered by assistant directors, who in turn have administrative assistants designated as section heads. Report-

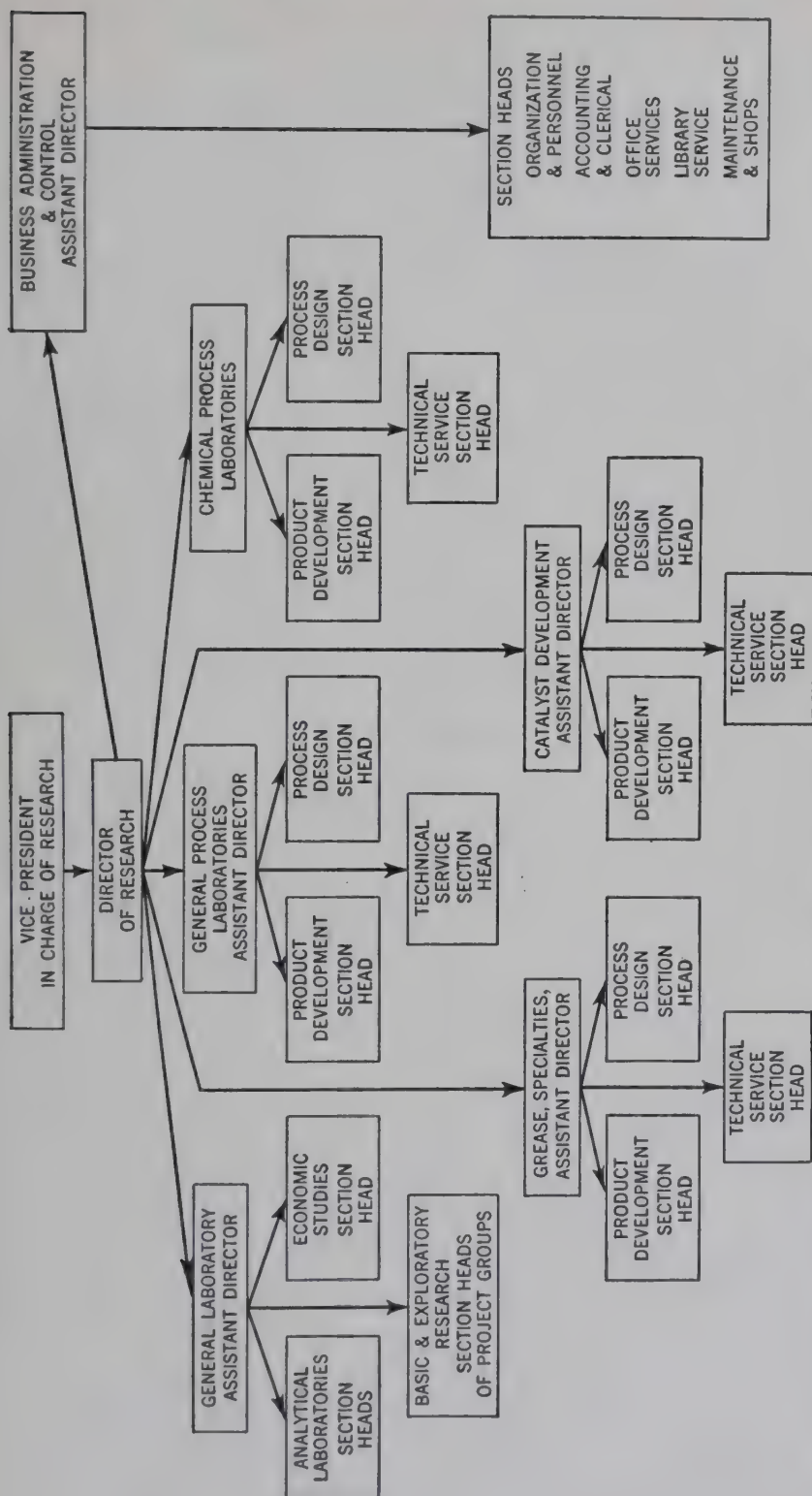


Fig. 14. Research organization of an oil company (simplified) (gross sales, approximately 500 million dollars). Research cost, 1948, approximately 6 million dollars.

ing to these section heads are a number of group leaders who may be in charge of one or more projects.

Economic studies, analytical work, and all exploratory research are done in the general laboratory division. Accomplishments of this division are passed on to the functional divisions, each of which has three sections: product development, process design, and technical service. The functional divisions are in a position to carry on a development to its logical conclusion. This arrangement has definite advantages in the large-scale organization, with regard to the problems of collective activity and communications. It is an application of the team approach on a successively smaller scale, down to the individual project groups. If advantage is taken of the fact that transfers of personnel may be made from the exploratory groups in the general laboratories for work in the functional divisions, the organizational relationships are extremely flexible. More elaborate analysis of this large-scale organization would be of little value for our purposes.

Summary

It should be apparent that no one organization chart is suitable for all research organizations. We have shown that organizations can be built up *functionally* from the research worker, his supervisor, through various levels of administrative direction to top management. A particular organization should be established on the basis of the requirements of the individual enterprise and the problems which it wants solved. Examples of various operating research departments have been analyzed. It has been demonstrated that the administrative pattern will be colored by the emphasis placed upon the various aspects of such activity in the individual company. The importance of the laboratory-level researcher in solving the designated problems, and the importance of his immediate supervisor in providing creative stimulation and desirable orientation, cannot be over-emphasized. The advantages of the team or project approach in providing for the desired objectives have been noted. Whatever the arrangement, clean-cut channels of communication should be available and clear lines of authority designated. Then, the resources necessary to solve problems can be provided, and the responsibility for their solutions clearly allocated.

CHAPTER XI

FORMAL AND INFORMAL RESEARCH REPORTS

Need for Communications

We have discussed the requirements of communications among the members of a research organization. We have noted the difference between informal and formal reporting and the necessity for both. The information on which action may be taken should be transmitted to those who need it with a minimum of distortion and in a readily recognizable form. It must also be available when it is required. There is little doubt that orally transmitted information can fulfill the requirement of speed of transmission. Whether it meets the other conditions necessary will depend largely upon the individuals transmitting and receiving it. Perhaps a given individual will be able to report orally upon a particular situation with clarity and conciseness. The recipient of this report may not understand it completely, may not remember it, may not recognize it as being of any importance, or may not be the person who should have received it. Or these difficulties may be the fault of the person making the report. Further, the oral report lacks the quality of exact reproducibility, which is most important from the standpoint of rational research activity, as well as the process of obtaining and protecting patents. For these and other reasons, the written report in any one of a number of forms is an indispensable part of research. A rational system of making such reports, based upon the requirements of the process, is a logical part of any pattern designed to increase research efficiency. As with the problem of organization, we shall find again that no inflexible and complete schema can be established which will be applicable to more than a few enterprises. Certain general types of reports may be distinguished, and the need for them determined in given organizations.

Of course, all reports, whether submitted on preestablished forms or written according to the desires of an individual, should

be in good style and follow the fundamentals of sound report writing. In the one case, the forms must be properly designed, and in the other, the individual must be educated to follow the desired principles. We may distinguish as distinct types: (1) those reports submitted on specific forms and (2) those in which individual latitude is allowed in the style and data reported. It is the responsibility of the research administrator to ensure that both are properly provided for in his organization. It should be noted that unnecessary and unused reports represent wasted time on the part of both transmitter and recipient. Their writing and transmittal are inefficient and should, if possible, be avoided. It is undoubtedly better to have too many reports, some of which may be superfluous, rather than too few.

We may further classify information which must be transmitted in writing in a research organization in terms of the type of data submitted, and the use to which it is put. Certain information will be necessary for administrative purposes; other material will be required in carrying on the technical activities of the group. A classification is also possible with regard to the organizational level from which the information emanates and the level to which it is directed. A report from an administrative director to management concerning the technical feasibility of a proposal would have different requirements from a memorandum issued by a group leader to his research team. We shall consider each of these categories and attempt to formulate general principles from which an efficient system of written communication in a particular organization may be established.

Forms

Forms for the reporting of information may be defined as pre-established stereotyped means for transmitting specified data, the nature of which is not left to the opinion of the writer. These data are usually quantitative, although in some cases forms may be utilized to obtain answers to questions of judgment. We shall distinguish between *the form* of a report and *forms* for reporting data, although both data forms and reports may be completely stereotyped and transmitted in a preestablished format. Forms in the latter sense would include questionnaires in which the only answers admissible were either "yes" or "no." On the

other hand, if such a report left the nature of the reply to the opinion of the writer, it would not be construed as a form in the sense we are discussing. An example of a typical form would be a report of time expended by an individual during a given period

| WEEKLY TIME RECORD RESEARCH DEPARTMENT | | | | | | | | |
|---|-----|-----|----------|-----|-----|-----|-----|-------|
| OF _____ | | | IN _____ | | | | | |
| Name | | | Division | | | | | |
| For Week Ending _____ | | | | | | | | |
| Project Number | MON | TUE | WED | THU | FRI | SAT | SUN | TOTAL |
| | | | | | | | | |
| | | | | | | | | |
| | | | | | | | | |
| | | | | | | | | |
| | | | | | | | | |
| | | | | | | | | |
| | | | | | | | | |
| Total Hrs. Worked | | | | | | | | |
| Absences(explain): | | | | | | | | |
| | | | | | | | | |
| | | | | | | | | |
| TOTAL | | | | | | | | |
| <div style="display: flex; justify-content: space-between;"> _____ Signed _____ Approved </div> | | | | | | | | |

FIG. 15. Form for reporting research time allocations.

on specified research projects. Such a form might be similar to that shown in Figure 15. Whereas the actual *data*, i.e., the number of hours allotted to each project, would be left for the individual to determine, the nature of the report—hours vs. projects—would be completely predetermined. This form may also be classified as administrative in nature, originating at the working level and directed to higher authority. Other more complex types of information can be reported on such forms, provided they meet the test regarding the nature of the data transmitted. For example, a request for an appropriation for the

purchase of equipment might contain questions as to justification, possibility of deferring the expenditures, etc. If the manner of answering these questions were left to the individual writing the request, then it would not be a "form." If after each question such statements as "express in dollars" or "give number of years" were used, then this would be a form in the same sense as a time report. A combination type is perfectly permissible and may be quite useful.

If properly designed, such forms as these have definite value in a research organization. The first condition placed upon their use is that the data requested must be quantitatively repetitive. Thus, in the time report the coordinates of projects and hours worked are repetitive from week to week. The worker will not be asked to report his expenditure of effort in hours one week and degree of satisfaction obtained the next. Nor would it be anticipated that he would be asked to report these against projects for one period and general subject fields the next. The exact degree of quantitative repetition may vary considerably from one instance to another. The yearly budget may be set up upon such a form but should be comparable with a similar budget to be established a year later. In other cases, forms will be used to report research data taken perhaps every 5 minutes. The period involved is of little importance so long as there is repetition.

Another requirement of this method of communication in a research organization is that the information transmitted be *useful*. It is characteristic of forms in general that they continue in use long after the data they were designed to record, transmit, and preserve have any functional value. An organization ordinarily does not remain static, and changes in both internal and external relationships have their effect upon the usefulness of information being gathered and reported. This is particularly true where forms are used. An advantage of forms lies in the fact that material contained in them is quantitatively repetitive and may be used for purposes of comparison on that basis. Nevertheless, a given form is relatively inflexible and can transmit only the information elicited by its design. It makes no difference that the data are insignificant or irrelevant; as long as the form is used, it will continue to furnish the same kind of information.

The test of the value of a form to a group is not whether it is being filled out periodically, but whether the data collected are being utilized. Individuals can be directed to fill out forms and even to manipulate the material thus obtained but, if no useful purpose is served, they will not functionally utilize them. It is possible that data are of value and are significant, but are not being utilized because of ignorance or other reasons. In any case, it is the responsibility of research administration periodically to examine the forms used within the organization and determine the uses to which the information collected is being put. Collection of data represents an expenditure of time on the part of some members of the group, and time spent on unnecessary or unused information is most inefficiently employed.

A great advantage of forms over other types of communications is that collection and manipulation of the quantities involved may be done on a routine basis. However, this is not possible unless their design is such that ambiguity is avoided. This is a most important requirement, and all forms should be carefully examined to ascertain that the information desired is requested *clearly, exactly, and concisely*. If this cannot be done and if the individuals furnishing the information are left with any doubt as to just what is required, then either the use or design of forms in the particular instance is improper. For example, a form requesting research workers to estimate progress on various projects in terms of percentages is of little value unless they are fully aware of what such percentages mean.

It is seldom, if ever, possible to use forms for the transmittal of creative data. They are particularly useful for purposes of control. In designing forms, this aspect of their utility should be carefully considered. In some instances, analyses of the material they contain may serve purposes other than of control. This is apt to be relatively rare for routine data collection.¹ Since forms in research organizations are either technical or administrative guides, or check points, their routing should be only that which is required to accomplish the purposes for which they were designed, and no more. For example, in a laboratory with a large personnel, it would be of little import to have individual

¹ Technical data may be used for creative work, but it is extremely rare to find administrative information which lends itself to this purpose.

time reports issued to the research director. They would be of use to the group leaders and intermediate administrative levels of authority. The research director would be able to utilize a summary report, such as shown in Figure 16, for control purposes. A report such as this would summarize the individual reports and indicate to the director what work was being done on each of the projects in the laboratory, as well as a total of the

| SUMMARY OF PROFESSIONAL HOURS CHARGED TO PROJECTS RESEARCH DEPARTMENT | | | | | | |
|--|--|--|--|--|----------------|------------|
| For Period Ending _____ | | | | | | |
| Division Project No. | | | | | TOTAL | |
| | | | | | This Period | To Date |
| | | | | | | |
| | | | | | | |
| | | | | | | |
| | | | | | | |
| | | | | | | |
| | | | | | | |
| | | | | | | |
| | | | | | | |
| Total Hrs. Worked | | | | | | |
| Absences, etc. | | | | | | |
| TOTAL | | | | | | |
| Forwarded by: _____ Sent to: _____ | | | | | | |

FIG. 16. Summary form for time chargeable to research projects.

hours charged against them to the date of the report. Additional forms for research organizations will be discussed later.

In summary, forms should be used where the communication of information, which is quantitatively repetitive, has utility. The format of the charts for collecting and transmitting such data should be as simple as possible. Misunderstanding as to the quantities to be reported must be avoided by careful design. Such forms should be routed only to those in the group who can utilize the information profitably. Obsolete forms should be weeded out and the design of all forms examined periodically for functionality. Routine reports made by individuals in the organization, for which forms are not used, should be scrutinized to determine whether the information might be transmitted more efficiently in a preestablished format.

Writing Reports and Memoranda

Where material communicated is not routine or quantitatively repetitive, individualized memoranda and reports are used. Certain general principles of form and style may be recommended for all such reports to render them most effective. Reports are generally distinguished from memoranda by their formality. A report is a more or less complete account of a particular situation. A memorandum is more closely akin to oral communication and might be said to be a written substitute for the latter. The same general rules apply to both, although it may not be necessary in the case of memoranda to include all the components we shall mention.

The prime requirement for all such writing, whether administrative or technical, is *purposiveness*. All reports and memoranda should be written with some objective, and this objective must be clearly understood by the author. In the case of reports, it should be stated in the text. For memoranda, where the writer knows definitely that the recipients understand the purposes for which they are written, this may not be necessary. In any case, the subject to be discussed should be quite clear. In a memorandum this may perhaps be accomplished with a proper title; a more formal report should include, in addition to a good title, a precise definition of the subject matter. The subject and the objectives should be carefully circumscribed so as to render misunderstanding of either on the part of the reader unlikely. If the writer of a report cannot clearly define his subject and state his objectives, then his report may be of little or no value.

Another important consideration in writing such material is the audience to which it is directed. This is all too often overlooked by the researcher. A report written by a physicist for his coworkers in the laboratory might be completely meaningless to a production executive. On the other hand, a report written for the latter would probably not be suitable for the transmittal of scientific information within the research department. In many cases a research paper is written for a mixed audience of technical and nontechnical personnel. Here it is often best to assume that the nontechnical readers will be interested in the conclusions *and their meaning* for practical operational problems in the

enterprise. The technician will probably wish to abstract from the report information which will be useful to him in his research work. Both types can be satisfied: the former by an abstract of the conclusions placed at the beginning of the report, with particular reference to their operational and administrative relevance, and the latter by complete and exact reporting of the technical situation in the body of the report. It has proved very useful in some research organizations to have technical reports forwarded to nontechnical personnel by research administrators with a brief memorandum covering the facts in language familiar to the readers. In some cases, the report should provide simple background information so that the reader may be able to make sense out of it. A wider experience than the usual researcher may be expected to have may be required if reports are to make the desired impression upon personnel in other areas of the company. It should be the responsibility of the research administrators to provide the advice and guidance to accomplish this end. If the organization is large enough, a staff of technical writers or editors may be utilized for this purpose. The advantages and disadvantages of this type of writing are discussed below.

We have already touched upon some aspects of the order of presentation of material. The reader should be able to follow the presentation quite naturally and, if the report is well written and the deductions are logical, arrive at the same conclusions as the author. There should be *no gaps* in the train of thought presented. A report should not begin to build up to a particular conclusion and then suddenly change to another argument, leaving the reader quite unaware of any intentions in this connection. The data, their manipulation, and hypotheses made from the relationships deduced should be relevant to the subject matter under consideration. Side issues should not be allowed to detract from the major argument. Where such matter does have pertinence, it should be included in the order of presentation in such a manner as to reinforce the conclusions reached rather than confuse the issue. The author should keep in mind that the reader is not so familiar with the details of his work as he is. What may be fully apparent and clearly logical to him, because of his intimate familiarity with the situation being reported, may be

completely foreign to his audience. He may avoid confusion by including in the report all transitional material required to arrive at a conclusion from a set of premises.

Where the objective of the memorandum or report raises questions which cannot be answered by the data available, these questions should be included. To have the reader raise these questions in his own mind and find no reference to them in the material presented weakens the conclusions which are reached. It is better to indicate all the important questions which follow from the subject at hand and point out the reasons for their solutions not being available. In this way the report will have a greater claim to authenticity and authority. Of course, if the author cannot write with some authority upon the subject, then he should not write at all.

Writing style will vary from individual to individual. However, it should be the objective of the administrators of a research organization to educate all the members of the group to write with clarity and well-defined conciseness. As Whitney so excellently points out in this connection:

Undoubtedly there is a high positive correlation between good thinking and effective writing. The reasoning process of the research procedures should naturally lead to and merge into an adequate account of research activities. Ordinarily, such an account calls for straightforward exposition. But in places there will be description as well as some narration. The problem is to get clearly and in detail before the readers just what has been done throughout the investigation now completed, or it may be to reproduce for them with interpretations the items of a situation which has been dealt with. There is no place here for impassioned argumentation nor for the didactic method of persuasion; just an illuminating account of facts and larger generalizations discussed and interpreted is all that is necessary. And, as the unskilled reporter will find, this is enough to tax his English writing ability to its utmost, as he must have regard for basic principles such as coherence and emphasis.²

Report writing is not strictly mechanical, and researchers should be impressed with the fact that a great deal of creativeness is required if their conclusions are to result in *action* on the

² F. L. Whitney, *The Elements of Research*, rev. ed. (New York: Prentice-Hall, Inc., 1942), p. 409.

part of other individuals in the enterprise. The preparation of an outline in which the salient points to be presented are included, and analyzed, is a necessary first step in writing a good report. Examples of what the research director considers satisfactory reports should be circulated among the personnel and, if possible, the steps in writing these reports should be analyzed for their benefit. If the group is sufficiently large, there is good reason to have a manual of style written by a competent technical writer for the use of the researchers. The use of a common technical language, as well as a uniform style, will assist the larger laboratory in increasing the average readability and usefulness of its reports.

It is well known that the *appearance* of written material influences the attitude of readers toward it. Therefore, the mechanical format of any report should enhance the ability of its contents to lead the reader to arrive at its conclusions. An obvious point in this connection is legibility and ease of reading. Double spacing is preferable to single, where the length of the report allows. Legible typescript and a minimum of corrections (and uncorrected errors) are also most desirable. Distinct sections should be clearly separated and properly subtitled. Data should be presented in such a form that the relationships under consideration are readily apparent. Observed data and accepted relationships should be distinguished both from opinions and from the deductions leading to the conclusions. Various types of reports have different requirements with regard to format. Rather than leave this matter to the individuals concerned, it has been found that preestablished arrangements are advantageous in making the average report more readable. Let us examine some of the formats which have been found useful in research laboratories for the communication of technical and administrative information.

MEMORANDA

The informal memorandum is used to request information from other members of the enterprise or research organization and to answer such requests. It is also used to request or give authority to perform some activity, and to transmit brief technical,

administrative, or operating notes for informational and reference purposes. It is generally not formalized to any great extent. It is, however, wise to have such communications uniform,

INTERNAL COMMUNICATION
RESEARCH DEPARTMENT

To: Research Director References: Progress Report #456;
 RD-239-50

From: A. B. Jones Reference: ABJ-134-50 Date: 1/12/50

Subject: Installation of additional equipment
in chemical pilot plant.

1. You have requested information as to the advisability of installing additional equipment in the chemical pilot plant as recommended in Progress Report #A-56.
2. The need for this equipment is quite urgent. In my opinion it will not be possible to continue work on Project A-6 if it is not obtained. This is in agreement with the conclusions reached in Progress Report #A-56.
3. An informal estimate of the cost of this equipment and its installation, as obtained from the Engineering Department, is \$45,000. A complete estimate has been requested and will be forwarded to you by January 26.

s/ A. B. Jones

Copies: Engineering Department, R.A. Doe
Chemical Pilot Plant, A.S. Smith
Manager, Service Department, R.T. Brown

File: Project A-6
Estimates

FIG. 17. A typical memorandum.

throughout the enterprise if possible, and certainly within the research organization. The establishment of such uniformity will tend to ensure that all such memoranda will contain a *minimum* of information required for purposes of readability. A typical informal memorandum is shown in Figure 17. The requirements of this particular form are the following:

1. Name of addressee
2. Any previous reference information which is pertinent, such as the numbers of other reports or memoranda
3. Name of author
4. A reference number applicable to this memorandum
5. Date of this memorandum
6. Subject of this memorandum
7. Body of the memorandum, with numbered paragraphs for reference purposes
8. Signature
9. List of recipients of copies
10. Special file information

Although these requirements seem quite simple, if they are not *preestablished* it will be found that each individual in the laboratory has his own ideas as to the form in which they should be written. The particular format illustrated meets the tests of uniformity and readability, but there are many others which may be adopted to serve the needs of a particular organization.

Since requests for approval and evaluations of projects, and work assignments stemming therefrom, are the basis of most research work, many organizations have set up formal memoranda covering these items. Such memoranda are quite often pre-established forms for reporting data, with individualized reporting of opinions and conclusions derived from the information available. When approved, a request may serve as a work assignment, thus simplifying the work of the administrative group. Figure 18 is a simple project authorization form. When properly written with a description of the work to be done and an estimate of the cost, it is a request for an appropriation or authorization of the sum indicated to carry out the work described. Necessary identification and reference indications are provided, as well as recommended allocations of project charges. When approved by proper authority and returned to the requester it serves as a formal work assignment. A simple form such as this usually requires supporting material in the form of additional memoranda or reports indicating the feasibility and desirability of the work. It may be desirable to formalize such support, and the necessary approvals may be obtained upon such a form as shown

in Figure 19, which is a request for authorization of a new product development for a mass-produced item. Based upon the approvals, a project assignment similar to that in Figure 20 may

| RESEARCH PROJECT AUTHORIZATION | | | | |
|--|----------------------|---|---------|-------------------|
| Title: | | | Number: | |
| File Data: | Allocate charges to: | Date Requested: _____ Period covered: _____ months | | |
| Description of work to be done under this authorization: | | | | |
| Requested by: _____ | | | | |
| Estimated cost: | | | | Written by: _____ |
| Item | Hours | Rate | Total | Approvals: _____ |
| Professional | | | | Group leader |
| Technical | | | | Date _____ |
| Material | | | | Laboratory |
| Service items | | | | Research Division |
| Total | | | | Management |
| Contingencies | | | | |
| Total | | | | |

FIG. 18. Simple project authorization.

be necessary. A combination of (1) the analysis or evaluation, (2) estimate, and (3) work order, if applicable, is probably the simplest and most efficient for this class of memoranda. Such a pattern is exemplified by Figure 21, which is used for chemical product development. Similar forms would be suitable for process development, exploratory research, and technical service.

These communications serve both technical and administrative purposes but are primarily useful for administration.

All these requests and authorizations are essentially memo-

| AUTHORIZATION FOR PRODUCT DEVELOPMENT | |
|--|--------------------------------------|
| Product line _____ | Date _____ |
| New Product _____ | Redesign _____ Proposal Number _____ |
| Description of Product _____ | |
| Reason for Request _____ | |
| Probable Price Obtainable per unit \$ _____ | |
| Competitive Products: | Our previous |
| Company _____ Model _____ | Price _____ models: |
| Estimated Sales: First Year _____ | First 3 Years _____ |
| Estimated Production Rate per Month _____ | |
| Estimated Tooling Cost \$ _____ | |
| Initial Production Cost (per lot) \$ _____ | |
| Total Cost of First Lot \$ _____ | |
| Amortized over _____ | Number of Units _____ |
| Total Unit Cost of First Lot \$ _____ | |
| Estimated Normal Unit Cost After Initial Production \$ _____ | |
| Estimated Development Cost \$ _____ | |
| Desired Development Completion Date _____ | |
| Finished Model to be Built: Yes No | |
| Disposition of Model _____ | |
| Current Status | |
| Original Amount Budgeted \$ _____ | Balance to Date \$ _____ |
| Total Amt. Previously Authorized \$ _____ | Amt. Required to Complete \$ _____ |
| Expenditure to Date \$ _____ | Balance on Completion \$ _____ |
| Approvals | |
| Product Line Group Manager _____ | Engineering Group Mgr _____ |
| Family Sales Manager _____ | Engineering Section Mgr _____ |
| Committee Action | |
| Review with Committee after Preliminary Development | |
| Amount Authorized for Preliminary Development \$ _____ | |
| Final Release for Development up to Budgeted Amount | |
| Merchandise Manager _____ | Vice President _____ |
| General Sales Manager _____ | General Plant Manager _____ |
| Chief Engineer _____ | Controller _____ |
| Executive Approval _____ | Date Final Approval _____ |

FIG. 19. Product development authorization (front). [After J. B. Davis and M. M. Brandt, "Engineering Responsibilities in Creating New Products," *Product Engineering* (September, 1948), p. 81.]

randa in which opinions, judgments, and data are reported in a form which has been found necessary and useful in administering a research group. Where the information required on a particular subject warrants the asking of repetitive questions in every instance, they can be quite advantageous. Properly used, they can ensure that desired points are not overlooked. Numer-

ous cases where they will be found of value undoubtedly exist in every research organization. For example, reporting the disposition of a research project may be done on a formalized memorandum.

Requirements

Essential Sales Features

Other Desired Features

Performance

Standardization Requirements

| | |
|--------------|-----------------------|
| Mounting | Finish |
| Weight | Type No. |
| Dimensions | Styling Sketch Number |
| Power Supply | Used with |

Instruction Book Requirements

Development Model Requirements

Remarks

FIG. 19 (*continued*). Product development authorization (back).

The timing of memoranda obviously is not subject to specific rules. It may be said that a memorandum should be written whenever it appears desirable, and when it does, it should be issued as promptly as possible. A memorandum containing stale information is as useful as yesterday's newspaper—for record purposes only.

The distribution of memoranda is of some importance in any group. It is essential that technical information be circulated, but it is neither desirable nor necessary that every informal communication be distributed broadcast. The individual research worker should be free to write a memorandum upon *any subject at any time* he so desires. He should have the opportunity to give written expression to any of his ideas no matter how far-

| PROJECT ASSIGNMENT | |
|--------------------------------------|--------------------|
| Classification No. _____ | Proposal No. _____ |
| Assigned to _____ | Group _____ |
| Charge Number _____ | Date _____ |
| References _____ | Copies _____ |
| Title or Subject _____ | |
| Please carry out the following work: | |
| _____ | |
| Should be completed by _____ | |
| Authorized by _____ | |

FIG. 20. Project assignment.

etched. But these do not have to receive wide distribution unless they are considered important by his fellow workers and supervisors. A carelessly written memorandum may do the research group a great deal of harm if it is circulated to the other departments of the enterprise. But to restrict the creative worker to writing only upon those topics and ideas previously approved by his supervisor is a highly undesirable deterrent to his ability. Therefore, in any research organization, the writing of internal memoranda for the benefit of the researchers should be unrestricted. Distribution of memoranda to individuals outside the group should be subject to some type of supervisory review. This review may be quite as important in determining the proper recipients of a given communication as in preventing unsound or poorly written material from being distributed.

| | |
|--|--|
| PRODUCT DEVELOPMENT WORK ORDER FOR Project Title: _____ | Project No. _____ Budget Item _____ Date _____ |
|--|--|

1. Purpose and Uses:

2. Potential Market (Estimate values, ranges or limits where practicable).

3. Nature of Market?

4. Total Potential Market Yearly?

5. Geographic Scope of Market? Initial Annual Sales: Volume_____ Value_____

6. Probable Selling Price (f.o.b.)

7. Probable Cost (f.o.b.)

8. Competitive Situation?

9. Other Factors to be Considered?

10. Does Product Fit Our Line?

11. Our Natural Advantages?

12. Effect Upon our Other Products?

13. Patent Situation?

14. Annual Saving in Manufacturing Cost?

15. Value of Improvement in Quality?

16. Other Factors?

17. Period Covered?_____ Scheduled Completion?_____

18. ESTIMATE OF COST

| | |
|----------------------------|-----------------|
| 19. Labor (incl. overhead) | \$ _____ |
| 20. Materials | _____ |
| 21. Other | _____ |
| Total | \$ _____ |

22. Approved_____ Responsible for Project:
 (For Department) _____

23. Approved_____
 (For Research Organization)

(Use back of sheet to elaborate on above items as necessary)
 (Outline proposed work on separate sheet to be attached)

FIG. 21. Combination analysis, authorization, and work order. [From D. H. Voorhies, *The Co-ordination of Motive, Men and Money in Industrial Research* (San Francisco: The Standard Oil Company of California, 1946), p. 59.]

REPORTS

In addition to the various types of memoranda, communications in the form of reports are required of a research organization. These reports will record the accomplishments of the group. Perhaps the simplest and least formal of these is the so-called *status report*. As the name implies, this is intended to convey a picture of a given situation at a particular time. Where a project comprises several problems being worked on

by various groups, status reports from each of these groups can supply the research administrator with a full story of progress to a given date. It will not necessarily be written to cover material which reports outstanding progress, nor is it essential that the information communicated be such that action may be taken by higher authority. It may be useful in assisting the research administrators in reporting to management upon the prospects of making progress on a given project. It may also be utilized to distribute technical information among the members of the group in a more formal manner than the memorandum.

Ordinarily, there are no special timing requirements for the writing of status reports. They should be issued whenever the researchers or the administrators desire to communicate or *receive* technical information relating to the project situation. These reports provide a means of outlining the technical problems which are being faced in accomplishing objectives. Since they may be issued from time to time during the active period of a project, they do not need to cover the entire history of a problem but either (1) the information obtained since the last report was issued or (2) a specific phase of the problem. This information is intended to present the situation at a given period and therefore should be written expeditiously; *i.e.*, it should be written *and issued* within a short space of time. A status report covering a given situation as of a month previous certainly is useless as far as presenting fresh information is concerned. For this reason, such a report is usually less formal than others and subject to fewer restrictions with regard to style, format, and manner of presentation. Generally, the more formal and stylized a report is, the longer it takes to write, edit, and publish.

It has been found useful in some research organizations to confine the formalized part of a status report to a cover sheet, such as is illustrated in Figure 22. On this sheet is given the pertinent information regarding the subject of the report, the time period covered, references, and objectives of the particular report. A summary of the information included, the conclusions reached, status, and recommendations for future work give technical administrators the material they require concerning the status. The body of the report then may be written in an informal, memorandum style, thus allowing these reports to be written and issued quickly. It may also be desirable to number

the pages of status reports consecutively during the life of a project. Space is provided for this purpose on the cover sheet shown in Figure 22. Thus, these reports will collectively represent a more or less informal history of the work done. The dis-

| | |
|---|------------------------|
| RESEARCH AND DEVELOPMENT DEPARTMENT | |
| PROJECT STATUS REPORT | |
| Project Leader _____ | Sanction No. _____ |
| Period Covered _____ | Pages _____ Date _____ |
| Title of Project: | |
| Object: | |
| Summary: | |
| Recommendations or Outline of Future Work: | |
| Status of Project: | |
| Approved _____ | |

FIG. 22. Cover sheet for status reports.

tribution of status reports is generally within the research organization only. Any information to be supplied to higher authority or other departments of the enterprise is usually abstracted and forwarded by the research administrators. This is not a hard and fast rule, but their very informality precludes the desirability of their being issued outside the confines of the research group. Status reports are also important as patent rec-

ords, serving to *date* the information and conclusions reached, as well as adding to the material contained in the researchers' journals and notebooks.

The next step in reporting the technical achievements of a research organization may be termed the *progress report*. This is generally used for communicating information as to progress made, to management and other departments in the enterprise for both administrative and technical purposes. Appraisals, evaluations, changes of objectives and emphasis, and other forms of action are ordinarily intended to be made upon the basis of the material contained in these reports. Where applicable, changes in production or sales procedures may be recommended in advance of completely attaining the objectives of a research project. Since such reports are intended to convey information on which action may be taken, it is generally desirable to subject them to careful review and analysis before issuance. They will take time to prepare and, when distributed, cannot be considered the most up-to-date information available. In some cases, work on a project may be interrupted while a progress report is being prepared and evaluated.

These reports should be written at those stages in a project when definite conclusions have been reached—in other words, when some specific progress has been made. They should stand on their own and enable the nontechnical reader to arrive at an understanding of the work that has been done and what is yet to be done. The technical reader should be able to evaluate the data, results, and conclusions and appraise their validity. For these reasons they should contain all the elements of a sound and complete report. These are generally considered to be

1. The title page
2. Abstract (often included on the title page)
3. Table of contents
4. Introduction
5. Body of the report
 - a. Prior history
 - b. Type of (qualitative and quantitative) information utilized
 - c. Means used to collect the data

- d. Presentation of relevant data
- e. Interpretation of data, with information as to their significance
6. Conclusions
 - a. Supported by the data available
 - b. Extended or extrapolated
 - c. Recommendations for future action
7. Appendix, if necessary
8. References and bibliography

The title page of such a report should contain sufficient information so that the reader will understand what material the report intends to cover without the necessity of reading farther. It should also contain all material necessary to identify the source of the report and properly file it. A special title sheet is usually found useful in the research organization to accomplish these purposes. Such a sheet is illustrated in Figure 23. This sheet contains the name of the author or authors, the title, reference information, a very brief abstract, and administrative approvals.³ The abstract on the cover should be quite short and cover only the general high lights of the material in the report.

The table of contents, of course, enables the reader quickly to find any section of the report in which he is interested. However, in addition to this it serves as a brief outline of the overall plan of the report. It should be written with both these functions in mind and should be complete enough to have some utility. In order for the table of contents to be complete, it is necessary that the report be properly divided, since the table is made up by merely extracting the headings from the text and arranging them in outline form.

As we noted previously, it is good practice to have the introduction contain enough information so that it is unnecessary for administrative and nontechnical readers to read beyond it to obtain the basic information and conclusions. The introduction serves an entirely different purpose than the brief abstract on the title page, which is intended to enable the reader to determine whether he is interested in this particular report at all. The

³ Some laboratories have found it convenient to have different colored cover sheets for the various reports for easy recognition and filing.

section. The briefer it can be made the better, within limits of coherence and clarity. Perhaps 10 per cent of the length of the body of the report should be devoted to introduction, although this is not intended in any sense as a hard and fast rule.

The body of the report will vary depending upon the nature of the subject under consideration. However, its function is to present in detail the information which has been accumulated. Subheadings should be used to enable the reader to follow the ideas presented easily and logically. Acknowledgments to reference and other material not actually collected in the particular investigation being reported on should be made properly. Since the object of the report is to present conclusions reached from data (both quantitative and qualitative), the sources and means of obtaining that data should be clearly outlined. The data should be presented with a view to their relevance and validity, and any interpretations should then follow logically.

The conclusions should be divided into those clearly supported by the interpretations of the data previously presented and those extended or extrapolated by intuition or "educated guessing" from them. There is no inherent evil in using a report to present such unsupported conclusions; the only danger lies in presenting these as though they were derived formally from the information available. Based upon some interpretation of the validity of the conclusions and the range over which they may be expected to hold, the report should present recommendations for future research. It may point out that the project has less hope of success than had originally been assigned to it and should be abandoned, or that a new and more promising avenue of investigation has been opened up and the work should be intensified along those lines. It may also point out where the results of the work to date may be useful in other departments of the company and recommend that steps be taken to utilize them. Whether positive or negative, the recommendations should be stated unambiguously, so that management, in the belief that it is following recommendations of the research department, does not put into effect decisions which were not the intent of the report at all.

The appendix should be used when there are additional materials which may be pertinent to a broader understanding of

the information presented but which are not essential to the conclusions reached. It includes the actual data sheets from which the information presented was abstracted, as well as similar items of reference material. The references should pertain to acknowledgments of source material properly indicated in the text. The bibliography may be internal to the organization (memoranda and other reports) and include a listing of other information on the subject. Whether a report should contain a bibliography will depend upon the subject and the nature of the report itself.

When a project is completed in the research organization, we have seen that there exists the problem of translating it into profitable use in that area of the enterprise for which it was intended. One of the aids in accomplishing this is the *final report* of the research group. The final report may be used by the engineering, sales, production, or patent departments of a company and should be written with this audience in mind. Whether it is to be used for transforming a pilot plant development into a production process, or (by the sales department) for promoting a new product, or (by a production unit) for improving the quality of an old product, it should be complete, comprehensible, and persuasive. It is not sufficient in such a report that the facts and conclusions be presented in a clear, concise manner—examples of their utility and recommendations as to how best to use them should also be included. The form of such a report would be similar to that described for a progress report, and the components would be subject to similar analysis. The major difference would be that this is the report ending the research group's work and presenting it to the remainder of the enterprise. Clarity and usability would be the keynotes here.

There is still another category of formal reports in the research organization which would follow the same general style and form as those already discussed. These are *technical reports* on subjects not pertaining to any project being pursued by the group. Not all organizations utilize this class of reports, relying on the memorandum to serve in this area. However, for purposes of distribution and authoritative writing, they have been found quite useful. Technical reports might cover meetings on a general topic within the organization, technical and society

meetings attended externally, visits to other companies, inspections of production processes, evaluations of the technical feasibility of proposals, and others. It may be found convenient to set up a special cover sheet for such reports, containing the very brief abstract and other pertinent information, and to have them written in the same form and style as the progress or final reports on work within the organization. In this manner, the other members of the enterprise will become accustomed to receiving them, and recommendations can be made to other departments.

Technical Writing Staff

The use of a technical writing or editing staff for reports in the research organization has been mentioned. Where the laboratory is large enough to warrant its use, such a staff has definite advantages in improving and maintaining a high standard of report writing. A technical editing and writing staff can usually be carried by an organization of 20 or more researchers, since the time saved in report writing and rewriting can be justified by this number. The general procedure followed is to have the researcher write the first draft of a report, and then have the technical writer rewrite, the former being responsible for technical accuracy and the latter for style and readability.⁴ In order to obtain the maximum efficient use of a technical writing staff, there are other duties which may be assigned to them, the following being a list compiled by Gray:

1. Abstracting incoming reports for the benefit of technical staff members
2. Reading incoming reports and routing them automatically to technical staff members
3. Indexing and cataloging incoming reports
4. Supervising printing, binding, and distribution of the laboratory's own reports
5. Carrying out reports of research work such as preparation of bibliographies, summaries, and so forth
6. Supervision of associated activities in the laboratory⁵

⁴ D. E. Gray, "Research Reports—Their Form and Usefulness," *Proceedings of the Conference on Administration of Research* (State College, Pa.: 1947), p. 51.

⁵ *Ibid.*

Additional Reports

The progress, final, and technical reports discussed have, of course, been concerned with the scientific research work of the group. As we have noted, there are also reports which are concerned with administration. These generally comprise either memoranda or forms. The use of memoranda for these pur-

| | | | | | | |
|-------------------------------|--|-------------|------------------------|-------------------------|--------------------------|------------------------|
| To the REQUISITION DEPARTMENT | | | | RESEARCH DEPARTMENT | | No. |
| Please order the following:- | | | | REQUISITION No.PLA | | APPROPRIATION No. |
| PROJECT No. | DWG No. | SECTION No. | SECTION | | ORDERED BY | |
| | | | | | | APPROVED BY |
| QUANTITY AND UNIT | DESCRIPTION | | | | | |
| | IMPORTANT -SPECIFY SIZE, MATERIAL, CATALOGUE, DRAWING OR REFERENCE | | | | | |
| | | | | | | |
| | | | | | | |
| | | | | | | |
| | | | | | | |
| | Previous Supplier | | LAST P.O. No. | | | |
| | | | | | | |
| | Price: | | QUANTITY ON ORDER | | | |
| | | | INVENTORY | | | |
| | NEEDED BY (DATE). | | RECEIPTS CURRENT MONTH | | | |
| | TO BE USED FOR | | USE CURRENT MONTH | | | |
| | | | INITIALS | DATE | ESTIMATED USE NEXT MONTH | |
| | COPY TO | | | | | |
| | | | | | | |
| | DELIVER TO | | | | | |
| | | | | | | |
| | | | | | | |
| | | | | | | |

Fig. 24. Requisition for research material.

poses has been considered in some detail, as have the design and utilization of forms. Such forms as those illustrated in the earlier figures are basic to the administration of complex collective research activity. Other examples of forms used include (1) requests for material (Figure 24), (2) periodic reports of research costs (Figure 25), the progress report chart shown in Chapter VII, and various types of work-load and personnel charts (Figure 26). There are, of course, many others which could be shown. The need for these will vary in each organization, and these examples are not intended to imply that the forms shown are the best or hold for every case. An analysis of the admin-

GENERAL RATING SHEET

Employee

JOHN DOE

Age

36

Service

6 Years

Duties

Senior Chemist - Organic Team

Department

Research and Development

I-VALUE ON ASSIGNED WORK

Rate the employee on his productivity for the Company as compared with the opportunities presented by his job. You should compare the things he does which are of value to the Company with the opportunities which he has.

Exceptionally High

Excellent

Good

Average

Indifferent

Value on assigned work.

10

9

8

7

6

5

II-CAPACITY FOR FUTURE GROWTH

Consider this employee's capacity for growth in the Company. Is the work he is now doing the limit of his capabilities or could he do work of increased scope and difficulty? Is he just about equal to the demands of his present position or has he capacity for increased responsibility? Age, health, mental ability, personality, character and record for accomplishments are all to be taken into account. Check one or more of the phrases which most accurately describe his capacity for future growth.

a-Should advance rapidly.

b-Shows promise of future growth beyond present job.

c-Could handle work of increased scope on present job.

d-Limited to present occupation.

e-Decreasing in efficiency.

f-Unsatisfactory for present occupation.

Fully qualified to replace his immediate superior.

Present job requires his full ability.

Very competent man but limited to present job on account of age.

Merits further consideration.

III-APTITUDE AND LEADERSHIP

Check the proper line to indicate the kind of work the employee could do successfully. More than one kind of ability may be checked. The position of the check-marks from left to right should indicate the degree to which he possesses administrative ability.

Marked Capacity as an Executive

Some Supervising Ability

Individual Worker Only

Laboratory

Engineering

Research

Production

Other

(describe)

(Over)

Fig. 26. Personnel rating chart.

istrative problems of a research group will determine the need for and the utility of forms, and an analysis of the design of the forms themselves will determine whether they are functionally proper in accordance with the general principles laid down earlier.

IV—PERSONAL TRAITS

| Exceptionally high | Excellent | Good | Average | Indifferent |
|-----------------------|-----------|------|---------|-------------|
|-----------------------|-----------|------|---------|-------------|

a—KNOWLEDGE OF JOB:
Mentality, Training and Expertise—Consider the extent to which he has and applies the mentality, training and experience to perform fully all the functions of his job.
Analytical Ability—Consider his ability to grasp essentials, reach sound conclusions, and plan necessary action in evaluating problems and analyzing data effectively.....

b—ABILITY TO GET ALONG WITH ALL KINDS OF PEOPLE:
Personal Requirements—Consider his effectiveness in getting along with subordinates, equals and superiors by the application of tact, fairness, co-operativeness, and skill in presentation.....

Supervisory Ability (Executive responsibility)—Consider the leadership displayed in organizing effort of others and in supervising, inspiring, selecting or developing others.....

—ABILITY TO GET RESULTS:
Initiative—Consider his accomplishments through tenacity, resourcefulness, ingenuity and aggressiveness.....

Cost and Expense Consciousness (Monetary responsibility)—Consider his ability to control and reduce expenses and costs by assuming responsibility for and making sound decisions affecting money, materials, economy of product, equipment and safety.....

Dependability and Accuracy—Consider his accuracy, thoroughness, and reliability.....

Endurance—Consider the degree and effectiveness of his mental and physical effort.....

COMMENTS

What outstanding characteristics will help his advancement? Stamina and drive. Organizing and supervisory as well as experienced creative ability.

What qualities will hinder his future development? Lack of a broad technical viewpoint.

Give other pertinent facts which should be known concerning this employee Has been responsible for several of our major projects. Received a merit increase on December 1, 1949.

Date January 1, 1950

Rated by Supervisor

FIG. 26. (Continued.)

Conclusion

We may conclude by repeating that it is better to have too much communication in a research organization than too little. Every report actually used for an administrative or technical purpose is a necessary one. The problem is never one of too much communication, but of inadequate, inaccurate, or unused

reports, memoranda, and forms. A continuing examination of the means used to distribute the technical and administrative information necessary to maintain the laboratory as a dynamic unit in the organization structure will do much to prevent such a situation from occurring.

CHAPTER XII

RESEARCH FACILITIES—LABORATORY DESIGN, RESEARCH TOOLS, AND AUXILIARY SERVICES

Resources Required for the Researcher

Resources must be provided for the research worker in order to reinforce his ability to solve problems, and the nature of these resources will depend upon the problems to be undertaken. Just as the type of organization and the personnel to staff it depend upon the broad policy laid down for research activity by management, so the resources which are required stem from this same decision. However, it is not anticipated that top management will necessarily understand the specific requirements, nor that the research worker will be able to provide them for himself. This is a problem to be worked out on the basis of recommendations of the research administrators. There is no doubt but that *the type and quantity of resources provided will affect the efficiency of the process*. Therefore the research administrator should examine with care the facilities he is providing his workers, if he is desirous of obtaining suitable answers (with a high degree of efficiency) to the problems he is asked to resolve. This will apply both to what is actually available in going research groups, as well as to the provision of additional facilities.

Basically, the resources which must be provided for the individual worker include

1. Physical working space
2. Physical equipment and material
3. Personnel and equipment to supply
 - a. Required equipment and material
 - b. Data and information
 - c. Physical assistance

Each of these resources will be required to a greater or lesser extent, in a variety of forms, in different organizations. Clearly

the market researcher requires an entirely different type of working facility than does the organic chemist, whose requirements differ in turn from the engineering machinery research man. For this reason we shall not attempt to lay down specific rules to apply to each type of research organization but will examine each of the basic categories of resources and outline general principles which will be useful in a critical examination of specific requirements. The provision of resources in excess of requirements will not make for efficient operation, but the lack of needed facilities will be dangerous if it stands in the way of obtaining suitable problem solutions. In this, as in all other phases of this type of activity, it is wise to err on the side of excess. This does not mean that the proper facilities, even in excess, can be provided without careful analysis and thought.

The Workplace

A place to work is, of course, the primary requirement of any class of worker. The researcher especially must have space adapted to the type of work he is undertaking. The first consideration in providing this space is the determination of the amount required. The space requirement will vary, depending upon the type of research being done, but for purposes of general analysis may be classified as follows:

1. Space for sedentary workers
 - a. Administrators
 - b. Analysts
 - c. Clerical personnel
 - d. Designers
2. Space for laboratory workers
 - a. Creative
 - b. Analytical
3. Space for development workers
 - a. Equipment
 - b. Process
4. Space for service personnel, and storage

Most of the personnel of a research organization fall into one of the above categories, and a general picture of the over-all requirements may be obtained if they are so classified. It is

recognized that there are many cases which will have special requirements apart from those to be outlined, but a knowledge of the general situation will also be of assistance in providing for these.

On the average, the sedentary worker, most of whose work is of course performed at a desk or table, probably requires the least space of any of the classes of research worker.¹ However, in determining the area to be provided for such workers, their duties and functions must be carefully examined. For example, we may say that the average clerk requires 70 square feet of space. If the duties of this clerk include filing for a group of researchers, space must be added to this area for the necessary file cabinets, etc.

The same situation is true of each category of worker. Thus, it is possible that the group leader or administrative supervisor may require more office space than the research director. The reason for this will be apparent as we continue our analysis. We have seen that the working level of research administration begins with the group leader. The space he will require, exclusive of the laboratory facilities which he will share with the members of his group, should be such as to provide him with the necessary privacy required for administrative duties. He should have an office and means for deliberative communication with the members of his group. An area of 140 square feet may be sufficient for his office, but this may be enlarged slightly to include room for a stenographer or secretary if no centralized pool of clerical assistance is provided. We have noted that this area should be near the laboratory where his group works to facilitate direct communication to the greatest extent. It was also pointed out that conferences between the leader and his entire group on a routine basis are necessary if the catalytic interchange of ideas is to be achieved. For this reason, additional space for *routine* meetings should be provided for the group leaders. This type of conference space is almost essential for the direct supervisors of any category of research activity. The conference area should provide about 15 square feet for each person in the research group. A laboratory which contains a minimum of such space

¹ We are speaking of *net* space, not including hallways, library space, lavatory facilities, etc.

or employs it all in only one or two conference rooms is probably handicapping itself unnecessarily. There is no estimating the value of such conference area, but in comparison to its cost (perhaps \$300 of capital investment per worker²) its provision is probably worth while. In the larger organizations, it may be desirable, in the interests of economy, to have several groups share these spaces. In this case, it would be sufficient to provide conference rooms so that each group may have the use of one for a full day per week—in other words, *one conference room for every five group leaders*. In addition to about 140 square feet for office space for the group leader and a minimum of 3 square feet of conference space per member of the group, based on sharing the space with four other groups, area may have to be provided for filing and clerical work if no central system is provided for this activity. We may use as a working figure 70 square feet per clerk or stenographer.

Higher administrative personnel in a research organization should be provided with office space of a similar nature, although individual preferences and general company practice may be allowed to govern here. Conference space should also be provided, although it is probably less important to have such space individual to each of the administrators concerned.

What we have termed an analyst is generally a researcher whose work comprises mental rather than manipulative activity—mathematicians, statisticians, stress analysts, etc. They may often have to be provided with space for more or less elaborate calculating equipment. Where they work in groups, it may be advisable to provide a sufficiently large space for the entire group. If no particularly elaborate equipment is used, 100 square feet per worker is probably adequate. However, it is not intended that this amount should be used in a single office—which would be too confining for most persons. It would be better to put two such workers in an office of 200 square feet.

The last class of sedentary workers includes the process and equipment designers who are often a part of the research organization. If their work is such that desks or drawing boards may

² In discussing building costs, we shall use an average figure of \$20 per square foot for comparative purposes. Building costs vary widely from year to year. At any given time, it is possible to obtain a value from reliable architects and builders for estimating purposes.

be placed next to one another, approximately 60 square feet may be sufficient area, otherwise the 100 square feet mentioned as an adequate figure for analysts may be required. Based upon all these figures, a general analysis of the space to be allocated to the sedentary workers may be made. It should be remembered that these are not recommendations, but merely starting points. In each case, an analysis of specific functions is required and space provided on the requirements thus determined.

The next category of research workers to be considered are those engaged in laboratory activities. These include chemists, physicists, engineers, biologists, etc., and obviously no single area can be given which would be suitable in all cases. Such workers have certain general requirements in common and, based on these, averages can be noted which will serve our purposes here. This class has been subdivided into creative and analytical (or routine) workers, since their requirements are somewhat different. In the case of the creative laboratory worker, space is needed to contain his equipment, to provide a working area around that equipment, and to provide for desks or tables for working with data and writing reports. Since the laboratory worker will often have one or two technical assistants, sufficient space to allow them to work in the same room is necessary. From 175 to 225 square feet are usually considered adequate to house a single worker, his equipment, desk, and assistants. Very often the areas for two to four workers are combined into a larger laboratory, and in some cases large open bays are provided for greater numbers. The latter practice, which is not common today, is based on the theory that communication and interchange of ideas are facilitated by common working spaces. Modern practice tends toward the provision of desk space within the laboratory area, which is more economical both from the standpoint of building size and time of the laboratory worker. Where separate desk space is utilized elsewhere, the laboratories may be somewhat reduced in size, and the considerations discussed for sedentary workers apply to the office space which must be provided.

The worker doing routine analytical work is usually housed in a communal laboratory, and here the space provided may be reduced to about 100 square feet per worker. He will ordinarily use standardized equipment, and his requirements are corre-

spondingly reduced from those of the creative worker. In general, the only desk space necessary in an analytical laboratory is that used by the section head or group supervisor. Special types of routine analytical work, such as tensile-strength testing of metals, require much greater areas per worker depending upon the particular equipment needed.

The space necessary for development workers, whether equipment or process, depends entirely upon the nature of the work. Development work ordinarily means using pilot or full-scale equipment which will vary from instance to instance. Therefore, the area provided for these workers is usually based not upon their number but upon the equipment to be housed. Process development units generally require more space than equipment development laboratories. In many cases, height, and thereby volume, are major considerations. Thus, one chemical process laboratory consists of a main process area 50 by 120 feet in size on each of three floors. An opening 11 feet wide cuts through the middle of the two upper floors to provide a central working space 40 feet high. Equipment includes percolation columns, tanks, distillation towers, reaction kettles, filters, driers, etc. Desk and office space for the researchers should, if possible, be provided contiguous to the working area.

The requirements of service personnel include areas for libraries, craft shops, storerooms, cafeterias, etc. Each research organization will have its own needs in this respect. A library should contain adequate area for quiet reading, for storage of books and documents, for abstract and file cards, for microfilm equipment if needed, plus office and desk space for the library personnel. No specific figures can be recommended for all cases, but working averages might be 200 square feet per 1,000 volumes in the library and an additional 100 square feet for each 50 professional personnel in the entire organization. Craft shops also depend upon the type of work being undertaken, but will not ordinarily require more than 10 per cent of the space provided for other purposes. Special and central storage facilities will perhaps not be more than 5 per cent of the total allocated to the remainder of the operations. Cafeteria space for the feeding of employees may be required, and a total working area for this purpose is 25 square feet per employee of any category. In addition to these requirements, provision must be made for hall-

ways, stairwells, etc., which will amount to perhaps 20 per cent of the over-all space.

The above general averages for space requirements of a modern research organization have been outlined for the purpose of providing an over-all picture for analyzing and estimating purposes. Each group will have its own specific requirements, but the figures given may be useful as points of departure. They are summarized in Table IX.

TABLE IX. SPACE REQUIREMENTS FOR RESEARCH ORGANIZATIONS

| <i>Item</i> | <i>Space required (approximate)</i> |
|--|---|
| Administrators..... | 140 sq. ft. (each) plus 3 sq. ft. of conference space for each member of the research group (minimum) |
| Clerical personnel..... | 70 sq. ft. each |
| Analysts..... | 100 sq. ft. each |
| Designers..... | 60-100 sq. ft. each |
| Creative laboratory workers, including assistants..... | 175-225 sq. ft. each |
| Analytical laboratory workers | 100 sq. ft. each |
| Development workers..... | Depends upon the nature of the equipment, but no less than that for laboratory workers |
| Library..... | 200 sq. ft. per 1,000 volumes plus 100 sq. ft. per 50 professional workers in the laboratory |
| Craft shops..... | 10 per cent of total space provided for laboratory and development workers |
| Storage facilities..... | 5 per cent of total space provided for other purposes |
| Cafeteria..... | 25 sq. ft. per employee |
| Hallways, etc..... | 20 per cent of total area |

Research Buildings

Research organizations may be housed in existing company buildings, in reconstructed buildings, or in new buildings designed and built for the purpose. There is perhaps little to choose from among these, if the space and service facilities are adequate to the requirements of the group. Where existing facilities have been used to provide room for research in the larger companies, it has often been found that these have become unsuitable after a period of time. For this reason, a great many of the enterprises employing more than 25 professional researchers have found it necessary to build new laboratories, or recon-

struct buildings designed for other purposes, to provide suitable working area for their research personnel.

The requirements of an organization change with time, and the policies they are intended to carry out are modified; as with other components of the production process, laboratories become outmoded and obsolescent. Modern design of research facilities has tended toward flexibility. The provision of space, per se, in existing buildings is often not difficult. The partitioning and rearrangement of such structures to meet the area requirements outlined above are quite possible. However, the broad needs of a modern diversified research group and the desire for flexibility often make the expense closely comparable with that of new construction. There have been cases where dwellings, school-houses, and even farm buildings have been remodeled to provide quite adequate facilities. In general, the after-the-fact consensus has been that new buildings could have been constructed for little, if any, more cost.

The smaller and more sedentary a research group is, the more likely is existing space to be satisfactory. A group of 10 laboratory workers can be suitably housed in a floor area of less than 3,000 square feet, and providing any ordinary services they might need is not apt to be costly. A market research group of any size can be adequately situated in existing office space. On the other hand, it is considerably more difficult to find space for a chemical process development group. An organization comprising several hundred people of various categories will almost of necessity require suitably designed buildings. Each case should be considered upon its own merits.

The provision of proper working space for a research group must be visualized as a capital investment for the purpose of supplying the individuals who are working (or will work) therein with the necessary tools for producing the solutions to problems. Adequate facilities may be more or less costly, depending upon the type of work to be done, but the greatest expense in this type of work is not the money used to provide these (or the salaries of the personnel working with them), but the failure to have solutions to problems when they are needed. The research worker can often ingeniously surmount deficiencies and obstacles in his environment, but imposing an initial handi-

cap upon him by virtue of inadequate working space or facilities is a shortsighted and extremely inefficient policy. Therefore, the prime consideration in evaluating new or existing locations as to their suitability for research should be whether they can or will reinforce and enhance the worker's problem-solving ability. Whether they do this by saving time spent in non-productive (or other than problem-solving) tasks, or by providing additional stimuli in the form of good light, cleanliness, quiet, etc., or both, is perhaps a minor issue compared to the recognition of this important factor.

Location of Research

Exactly the same point must be made in the controversial issue of where a research group should be located. There is no denying the importance of location; therefore, let us summarize some of the factors to be considered:

1. Location of plant or plants of the company
2. Location of administrative offices
3. Type of research to be undertaken
4. Educational and other institutions
5. Personnel supply and housing facilities
6. Climate
7. Location of other commercial research establishments

Often one or the other of these factors predominates in the choice of a suitable location; if so, the reason will generally be management's policy for the research to be undertaken. As is true with most decisions of this nature, it is usually wise to give some weight to all the considerations which will in any way affect the efficiency of the operation.

The location of the research group at or near a major manufacturing plant of the enterprise has definite advantages in keeping the workers and their administrators in close touch with production problems. Where there are several plants, some companies maintain research groups at each of them for this reason. Production problems are not always paramount in industrial research; in some companies *new-product* or *end-use* research may be far more important. Such research usually begins in an exploratory manner, and the ubiquitous pressure of plant person-

nel for the solution of their problems may severely handicap the pursuit of the activity. If the solution of both types of problems is important from a research standpoint (and as we have noted previously not all production problems fall into the research category), then it may be desirable to locate the latter away from the production facilities, and to maintain production research groups at the various manufacturing plants. We have already said that a research organization should not become a routine quality-control center, a position which is apt to be forced upon it by its proximity with a manufacturing plant. On the other hand, if properly controlled, these pressures may be used as stimuli to challenge the research group to accomplish the solution of important and difficult problems.

If the administrative offices of an enterprise are located at or near the main producing units, this is an added incentive for the housing of the research nearby. If top-management policy is to be the guiding influence in determining the course of research, then it is quite naturally advantageous to have the responsible individuals relatively near each other. In fact, it may be far more desirable to situate the research organization near the main administrative offices of the company than to its manufacturing plants. The process of undertaking new-product or end-use research can be made much more efficacious if this is the case. The viewpoint that proximity of administration to research encourages nontechnical "meddling" has no foundation in a properly administered enterprise.

The type of research to be undertaken is an important factor in location. There are cases in which it may be completely determining; for example, the fertilizer manufacturer who engages in agricultural experimentation and whose plants and administrative offices are located in urban areas. His laboratories are of necessity located at some distance from either of these. The solution of production problems with large-scale pilot plants may make it necessary that the research organization be located at a manufacturing unit, as has been noted. A market research group, on the other hand, may be handicapped if its headquarters are not situated near some fairly large center of communication and trade.

Since one of the administrative problems of research is to encourage and promote the ability of the workers to solve prob-

lems, complete isolation from professional and cultural institutions is undesirable. Such institutions may be utilized to give direct assistance to the organization in the solution of problems, or to enable their researchers to study and advance in their particular professional fields. Their mere proximity may provide stimulation and aid in individual achievement. The availability of cultural and recreational resources may also accomplish the same purpose.

These factors also have a direct influence upon the supply of trained personnel for the organization. Since, by and large, the universities serve as primary, and other research institutions as secondary, sources of supply, location at great distances from either of these can be a handicap in keeping the groups well staffed. In any case, in considering a new location for a research organization, it is well to ascertain that an adequate supply of the type of personnel desired is available and willing to work in the location chosen. The availability of housing facilities for the staff must also be considered in this connection. In areas near centers of population, this may pose no serious problem, but in isolated locations housing may have to be provided by the company.

Climate has its bearing upon the personnel situation, as well as upon the design and construction of laboratories. In hot humid climates, air conditioning is a necessity if the work is to be carried on efficiently. It is also desirable, and perhaps essential, in the more or less dirty atmosphere of most urban industrial localities. Climate is certainly not the most important of the factors discussed, but it has its effect upon the work done and the construction of the laboratories. For example, where large-scale pilot operations are undertaken, it may be economical to locate the unit in a climate where outdoor operation in open-air sheds is possible.

The presence or absence of other industrial research organizations is a minor factor, but one which might play an important role in certain instances. Their presence usually assures a supply of laboratory personnel, as well as an opportunity for the establishment and encouragement of professional societies and groups. Further, just as educational institutions stimulate the individual growth of researchers, so the presence of other laboratories encourages the exchange of knowledge and thereby in-

creases the possibility of achievement. Of course, there is the viewpoint that secrecy is more difficult to maintain in an area where there are competitive research organizations. However, the importance of such secrecy is more often than not overrated, and ordinary ethical considerations are probably sufficient to make most research operations safe from this standpoint. It is more important to be able to solve problems efficiently than to be able to maintain the highest degree of internal security.

The combination of all these factors will usually lead to a satisfactory choice of location for the laboratory or research organization. A composite choice would probably be, except in cases where a particular factor has overwhelming importance: (1) near, but not at, a major plant, (2) near, but not at, the administrative offices, (3) in a suburban area near a large center of population, and (4) in a temperate climate. That this is the case is indicated by the fact that approximately one-quarter of the industrial research expenditures in this country are now made in the state of New Jersey, which fulfills reasonably well all these conditions.

Laboratory Design

Once the amount of space to be provided and its location have been decided upon, it may be necessary to design and construct these facilities. As was mentioned earlier, the desire for flexibility is a strong motivating factor in design. Because it is usually difficult to predict future needs of an organization, the design should be such as to maximize the possibility of providing for future developments. There are two schools of thought in this connection. In one (and this is the older), the layout is planned to provide maximum facilities for existing functional groups on a more or less permanent basis. All present needs and such future needs as can be anticipated are provided for in the most efficient manner possible. If changes are required in the future, they will be made on a permanent (and costly) basis, or additional facilities will be built. The other, and more modern, school provides the concept of "universal space," which was first employed in the Bell Telephone Laboratories group.³ The prem-

³ "Happy Hunting Grounds," *Industrial Bulletin* 254, Arthur D. Little, Inc., May, 1949, p. 1.

ise underlying this concept is that space is usable for a multiplicity of purposes and can be rearranged to suit changing conditions if provisions are made to do so in advance. In such a design, the interior is divided by standardized partitions, which may be moved about to change space to any multiple of the basic module within a short time and at a relatively small cost. The great majority of the modern large research laboratories are built in this manner. The objections to such construction are the added cost of the movable partitions, the necessary duplication of services, and the somewhat inflexible nature of the modular space.

In the small laboratory, the disadvantages probably outweigh the flexible nature of the arrangement; the larger units, on the other hand, undergoing as they do more changes and a wider variety of simultaneous work, have been served most usefully by this type of design. It should be clear that the choice of the basic module is of great importance, whereas in the fixed design the provision of adequate facilities for each of the functional activities is the major consideration. Modules have varied in size to provide the various space requirements discussed earlier. It is to be expected that such would be the case, since the nature of the work done will to some extent dictate the dimensions. The Whiting Research Laboratory of the Standard Oil Company of Indiana has been constructed on the modular principle, but different basic modules have been provided for different purposes. For example, in the office building the basic unit is 10 feet wide and 14 feet long. (This is in accord with the usual practice of placing the short dimension along the outside wall to maximize the utilization of window space and reduce corridor requirements.) In the main laboratories, the corridors have been placed off center so that the single units are either 11 by 16 feet, or 11 by 20 feet. This off-center design provides two basic sizes, the smaller suitable for offices or for laboratories that do not require extensive equipment, and the larger for more elaborate installations. It is said that most of the laboratories in this building are made up of two 11-foot units, for use by two chemists. During the design of these particular laboratories, mockups of the modules were constructed for examination, use, and criticism by the research and other personnel. This appears to be particularly sound practice, since once such a building is constructed,

the basic space unit cannot be changed. Here also, the advice and assistance of competent architects are necessary.

Adequate provision of services, depending upon the type of work to be done at each of these modules, or in the case of permanent arrangements, in each of the functional areas, is necessary if the design is to be considered adequate. The combined analysis of the architect, research personnel, and company engineers is desirable to make sure that this is done. The same consideration holds in the case of providing specially constructed buildings for unusual purposes. An example of the latter is the gas research building of the B. F. Goodrich laboratories where a high-velocity stream of fresh air is continuously forced through the space.

Air conditioning and windowless buildings are particular requirements which must be considered and decided upon in each instance. The advantages and disadvantages of each of these are of the same general nature for research laboratories as for other facilities. It is often necessary to effect a compromise between technical and comfort considerations. Where particular climatic conditions must be provided for standard tests or other purposes, air conditioning is often a must. In laboratories containing a great deal of heat-producing equipment and fume hoods (since the air cannot be recirculated as a rule), air conditioning is apt to be quite an expensive installation. However, sound architectural and engineering design can keep to a minimum the cost of both air conditioning and providing services. The layouts which will prove to be most efficient will vary in each instance depending upon the particular requirements, and therefore none will be presented here. It is sufficient to note that, as with any other industrial building, a combination of sound architecture with adequate facilities to accomplish the work required, plus flexibility, is usually all that is necessary.

Cost of Research Facilities

The costs for constructing research facilities vary in accordance with going building costs. The price of a research building, without equipment, with permanent partitions is approximately the same per square foot as for any other industrial building of like design. The cost of a building with movable partitions

will be from 25 to 100 per cent more depending upon the size of the basic module and the services to be provided for each one of these. These costs ran from \$10 to \$20 per square foot in 1948. Equipment and other costs can add from 50 to 100 per cent to this figure. A figure for *total* investment in buildings and equipment for *each employee* in the research organization is given by Voorhies as \$6,000.⁴ It should be obvious that the amount expended for equipment will vary considerably depending upon the type of work being done and the requirements of the problems. \$3,000 has been noted as the cost of furniture for an average laboratory module, and \$3,800 as the investment in stock and special apparatus per research worker.⁵ We may repeat that, whereas economy is an important goal in designing and constructing research facilities, the provision of adequate working space with such conveniences as will enable the worker to solve problems more efficiently is the primary consideration. *Sound design and planning will be amply repaid in the long run.*

Miscellaneous Research Facilities

In addition to the physical working space and the furniture and apparatus of the laboratory, other equipment and material are required for the researcher. Although he may not use some of it himself, it is as much a part of his requirements as his laboratory bench. This additional material includes

1. Instruments and supplies
2. Raw and finished materials
3. Craft shop equipment
4. Literature and reference materials

Instruments and supplies to keep his day-to-day work moving will ordinarily be provided, either directly in his own working area's storage space, or by storerooms and stock rooms in the laboratory. Sufficient material should be available to the individual worker so that he does not lose any significant portion

⁴ D. H. Voorhies, *The Co-ordination of Motive, Men and Money in Industrial Research* (San Francisco: The Standard Oil Company of California, 1946), p. 53.

⁵ C. C. Furnas, ed., *Research in Industry* (D. Van Nostrand Company, Inc., 1948), p. 339.

of his research time either looking or waiting for it. Storerooms should be conveniently located near the research spaces. If it is possible, a system should be established so that the worker may requisition routine supplies directly from his laboratory, either in writing or by telephone, and have the material delivered directly to him by messenger. This may seem to be an unnecessary refinement but, when it is recalled that a messenger will cost in salary perhaps one-fifth of a professional worker, the savings possible are readily apparent. In addition, the use of messengers to deliver and call for instruments and equipment will reduce the required inventory of such material by eliminating the usual tendency to retain these items beyond the time needed.

The various types of research require different and varying amounts of supplies. Physical and chemical research needs small instruments and a multitude of chemical compounds. Pilot plant work requires material in bulk. Market research has little need for these types of items but may require considerable amounts of printed forms, questionnaires, etc. The research administrator should ensure that his workers are not handicapped by a lack of such routine supplies. They should be provided with a system of requisitioning them with a minimum of red tape. Clearly, the cost of the particular supplies needed will have a bearing upon this latter point. Thus, a requisition for material costing no more than \$10 should not need the approval of top administrative authority. On the other hand, a limit must be established for each of the administrative levels, so that proper control of the purchases of the organization may be maintained. These limits will of necessity be dependent upon the size, scope, and objectives of the group. A number of laboratories set these limits approximately as follows: research workers, \$50; group leaders, \$100; administrative directors, \$250; research director, \$500 or \$1,000; and greater amounts to be approved by management. This implies that no higher authority than that noted is required for the purchase of supplies within the given limits. These levels will, of course, be responsible to their supervisory personnel for staying within some budgetary limits. The budget should have been determined by the objectives of the individual workers and the groups.

Special equipment as opposed to routine supplies comprises such apparatus as is needed only in a specific type of work or for a specific project, and is not provided as regular equipment for the laboratory or working space under consideration. Such equipment may be either purchased, designed, and constructed by the organization, or rented. There is another alternative which should not be overlooked: the experiments or tests which must be run on such apparatus may be contracted for accomplishment by outside organizations. Over-all economic considerations must be taken into account in determining which of these courses should be followed. For example, the purchase of apparatus which is *readily available* for a given purpose is probably the most economical of expenditure and time. On the other hand, such equipment may not be quite suitable and will require modification, or it may be totally useless. Where apparatus is very expensive and where it will be suitable, it may often be rented. This is worth while if it will be useful for only one project or for a short time. In the same case, it may prove satisfactory to have the work undertaken by one of the many commercial or institutional establishments.⁶ This last-mentioned plan has the added advantage in particular cases of utilizing personnel who are familiar with the apparatus in question and avoiding the necessity of training or providing such workers within the organization. Such a consideration might apply, for example, to the use of an electron microscope by a small research group. The last alternative, and one which is not at all uncommon, is the design and construction of the special equipment within the organization itself. This is probably the most costly and time-consuming, but it cannot be avoided in many cases and, in some, is absolutely essential. The latter is often true in pilot plant and developmental research. For this purpose special personnel are needed and, in the larger laboratories, are provided in the form of research engineers and process equipment designers. The design of such equipment should be such

⁶ *Directory of Commercial and College Laboratories*, National Bureau of Standards, *Miscellaneous Publication M187* (Washington: U.S. Government Printing Office, 1947). Other listings are available, such as New York State Department of Commerce, *Directory of Research and Development Facilities at Educational Institutions in New York State Available to Industrial Concerns* (Albany, N.Y.: 1946).

as to provide not only means to acquire the necessary data but efficient means as well. We noted earlier the cost of the improper design of such apparatus. The requisitioning of such equipment is subject to the same considerations as noted previously. In this case, the personnel must be able to ascertain easily what is available and how it may best be obtained. Such information should be provided expeditiously by the service organization set up for the purpose.

If this type of equipment is to be constructed within the organization and if the material is to be maintained and improved for the researcher, craft shops are necessary, which should be provided with equipment of their own. If the laboratory is located at or near a plant of the enterprise, it is possible that the plant shops may be utilized for this purpose. However, experience has shown that to a production man, production is (rightly) the most important item for which any of his facilities can be used. If it comes to a choice between the maintenance of a piece of production equipment and the construction of something for the research group, the former almost always takes priority. The fact that expensive research personnel may be idle, or at least not fruitful, because of such delays is a minor consideration to the production staff. For this reason, the trend in research organizations has been to supply much of the needed services and equipment within the group. The investment in terms of research time saved is not as a rule very large, since \$50,000 to \$100,000 will ordinarily equip craft shops (machine, carpentry, electrical, pipe-fitting, metal smithing, etc.) most adequately.

The storerooms, purchasing organizations, craft and design groups must be suitably staffed if they are to be of any value to the organization. The better the personnel provided for these groups, the better and more efficient will be their service to the research laboratory. It should never be forgotten that they exist for this purpose, just as the research organization itself exists for service to the other divisions of the enterprise, and not vice versa. The number and size of such groups will depend upon the activities of the research department, but experience indicates that the proportion of such service personnel to researchers will ordinarily range from one to three to one to ten (not including the technical assistants to the research workers).

Storage facilities for, and means for dispensing and circulating, literature and reference material must also be provided. This, of course, connotes a library, but more than this must be maintained. The importance of a wide and well-rounded background in any subject under research consideration has been emphasized. The current and past technical literature and patents provide the base material for such a background. It is inefficient to expect a research worker to obtain all this information on his own. In any case, it is barely possible for him today to keep up with current information in his own particular specialty, much less maintain his contacts with other fields. The library and the staff who operate it in any efficient research group must maintain or have access to all the sources of information which would be of utility to a worker in a given project. For best results, they should be able to prepare bibliographies and abstracts of pertinent material *rapidly* and to furnish promptly the full text (translations, where necessary) of specific literature which the researchers feel would be of additional interest.

In addition to external sources of information, there is usually in any company a wealth of written material, specifications, operating notes, reports, etc., which can be of great value. If these are to be useful, they must be indexed and made available through the bibliographies we have mentioned. Abstracting of both external and internal information periodically and the publication of such information to the research staff have proved useful in stimulating the research worker and providing him with essential material in an easily digested form. Such abstracts may be prepared by the library staff or by the research workers themselves, under the direction of (and collated by) the library personnel. The first is preferable but probably represents the greatest out-of-pocket expense.

The choice of books and periodicals can be left to the worker, by having him request the library to purchase or subscribe to those he feels he needs. The extent of this service will depend upon the size and type of organization, but again the investment in terms of returns is relatively small. Even the smallest organization can usually afford to maintain subscriptions to 40 or 50 periodicals, and in the larger laboratories upwards of 100 would not be uncommon. In the very small research group, one good technical librarian can do much to get the required data at

the least cost, by borrowing books and periodicals, obtaining bibliographies, abstracts, etc., from large institutional libraries which are equipped to render such service.

Another type of informational service is that performed by analytical or testing groups. Such groups are needed for routine material analyses, instrument calibrations, and standard tests on equipment. In establishing such groups, operating costs must be weighed against the expense and loss of *creative time* involved in having the researchers perform the work themselves. The workers for such routine operations are undoubtedly less expensive to maintain than the creative personnel. For this reason, it may be profitable for the small organization, where the size of the group does not warrant separate analytical facilities, to have this type of work done by external commercial establishments.

In a sense, the accounting data necessary to administer an efficient group are part of the reference material required for operation. The staff and requirements for obtaining and distributing such material have already been described. Records must be kept and made available to the administrators to be used for planning and analyzing both current and long-range activities of the organization.

The accounting for all the types of equipment and services described is not a simple matter. If budgets are to be kept and funds allotted upon the basis of projects, records must be maintained to indicate the flow of the monetary expenditures into these projects. Except in those cases where the permanent equipment of a given laboratory is used for only one project, the depreciation charges on such apparatus would be extremely difficult to isolate. Therefore, they are ordinarily charged to the general laboratory expense or overhead and allocated to projects on the basis of expenditures or man-hours, or are shown as a separate item in the budgets as was noted previously. Routine supplies and instruments used are often handled in the same manner. The cost of maintaining project records would not be warranted by the value of the information obtainable as a general rule. In the case of standard supplies, an average value of inventory consumed during a given period is usually all that is necessary. The same consideration holds generally for staff and library services.

On the other hand, special equipment, usable in only one project and having a value of sufficient magnitude, may be charged directly to the project for which it was purchased or constructed. In this case, if the apparatus was constructed within the organization, the services of the craft personnel and the depreciation on their equipment may be validly allocated to projects. We may similarly distinguish between routine and special maintenance. Thus, if a particular project apparatus requires the full-time services of an experimental mechanic, it is logical to charge the project with the cost of providing him. Similar considerations hold for analytical work; charges may be established for routine tests and allocated to projects. The establishment of a consistent and usable system for these charges lies within the province of a competent accounting organization. The accounting group of the company, with the assistance and advice of the research administrators, can easily set up a proper system. The research administrator should keep in mind that what he needs is *information which will tell him what proportion of the organization's efforts is being expended in attaining the solution of specific problems*, and where the major items of cost are located. Too great a refinement is neither necessary nor desirable, but a picture of these costs which is relatively free from distortion is essential.

The last item of services which must be supplied to the research worker is physical assistance in the form of technical personnel. Many of the routine observations or manipulations which are required to solve problems can be handled by non-professional personnel. This is necessary both from an expense standpoint, since such workers are considerably less costly, and from a morale point of view, since the researcher is not happy doing routine manipulations or observations constantly. The number of such assistants that a given worker can utilize will, of course, depend upon the type of his work. This number cannot be determined satisfactorily upon the higher administrative level; the judgment of the worker and his immediate supervisor should usually prevail. The proportion of these assistants has ranged, in various laboratories and research organizations, from five for each professional worker (in a market research group) to one for each four professionals (in a physics group).

Summary

In summary, the research worker cannot operate alone in solving his problems, nor can he operate in an unprepared physical space. He needs services, supplies, information, and assistance, and these in varying quantities if his work is to be accomplished efficiently. Overhead is no small part of operating a research organization, ranging from 50 to 100 per cent on the cost of the professional worker but, if properly chosen and supplied, it can greatly enhance the efficiency of the group. We return again to the conclusion that *the greater the reinforcement and assistance supplied to the creative researcher, the greater his chances of solving the problems delegated to him.*

CHAPTER XIII

PATENT POLICIES IN RESEARCH

Patents and the Research Worker

Patents and patent pools, as well as large holdings of patents on the part of individual enterprises, have been much criticized as monopolistic and not in the public interest. Yet in a competitive economic system the use of industrial research to solve problems which will enable an individual enterprise to profit by the creative ability of its scientists and engineers is taken for granted. Where these solutions fall within statutory and legal definitions of "invention," the enterprise has the *right* to apply for patent protection. That a patent, *if valid*, gives the inventor a limited type of monopolistic right to prevent others from utilizing the invention for a period of 17 years, is ordinarily not questioned. If patents are used in restraint of trade or in unfair competition, there is, of course, justice in the prevalent criticism. On the other hand, our patent system was conceived by the writers of the Constitution as a device ". . . to promote the progress of science and useful arts by securing for limited times to authors and inventors the exclusive rights to their respective writings and discoveries."¹ Industrial research and the progress derived therefrom are certainly not completely dependent upon the patent system for their existence, but as with all other similar incentives, it has provided a great impetus for continued research and development.

One of the major by-products of successful collective industrial research is the evolution of patentable "inventions." These inventions, whether patented or not, are the bases of obtaining a return upon the research investment. In a sense, they must be paid for, they must be protected in one way or another from use by competitors, and they must, in general, be utilized in a profit-

¹ Art. I, Sec. 8.

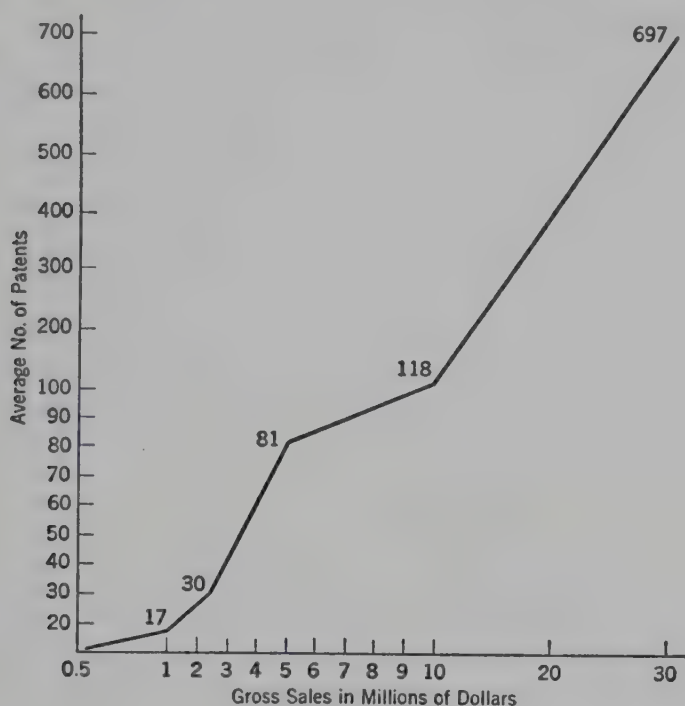
able manner by the individual firm. The patent is by no means the only method for accomplishing these purposes. It is entirely possible, if perhaps somewhat foolish, for a company to ignore the patent system completely. It could rely upon its speed and adaptability in profiting from the products of its research laboratories by utilizing problem solutions before its competitors could learn its arts. Or, a company might, if it wished, rely upon secrecy in preventing other firms from obtaining knowledge of its methods. This practice is not uncommon and is often combined with an aggressive patent policy to increase the margin of competitive advantage, since patents ordinarily do not divulge the minutiae of know-how which often spell the difference between profit and loss in industrial operations. However, at the present time, the holding of patents seems to be the most common method of attempting to protect the results of research activity in industry.

A recent survey shows that some 371 representative companies hold an average of 19 unexpired patents each.² It is not surprising to find that the larger firms hold a far greater average number of patents than do the smaller concerns. Figure 27 shows the average number of patents held in firms of varying sizes, as determined in this same survey. The increase in number of patents with sales volume follows roughly the same pattern as do research staffs, as shown in an earlier chapter. There comes a point in the size of research organizations where the efficiency of the "mechanical" process of obtaining a series of patents hinging upon one primary invention increases at a rapid rate. This is due to the ability of the larger organization to explore many of the fringe fields surrounding the area of a given invention, and to obtain patents based upon the "novelty" of being able to manipulate the concepts involved in this discovery from the standpoint of these fields. An example of this is the large number of patents relating to nylon held by E. I. duPont de Nemours and Company.

The research administrator is ordinarily not directly respon-

² National Association of Manufacturers, *Trends in Industrial Research and Patent Practices* (New York: National Association of Manufacturers, 1948), p. 66.

sible for the obtaining of patents based upon the results of the work of his organization. Patent law is a highly specialized and



| Sales, dollars | Av. No. of patents | No. of firms in class |
|-----------------------|--------------------|-----------------------|
| 0-500,000 | 15 | 27 |
| 500,000-1,000,000 | 15 | 32 |
| 1,000,000-2,500,000 | 17 | 57 |
| 2,500,000-5,000,000 | 30 | 76 |
| 5,000,000-10,000,000 | 81 | 60 |
| 10,000,000-30,000,000 | 118 | 76 |
| 30,000,000 | 697 | 78 |

FIG. 27. Average number of unexpired patents held by industrial concerns of varying sizes. Sales volume is plotted at the lower limit of ranges. (From National Association of Manufacturers, "Trends in Industrial Research and Patent Practices.")

complex branch of the legal profession, and it is not sound practice to expect that the research executive is capable of handling this work. However, the success of the patent attorney and the strength of the patents he can obtain will be dependent upon his working cooperatively with the research department. There are

many steps which the research administrator and worker can take to enhance the measure of protection a company can obtain under the patent statutes. The research administrator may also be called upon to analyze and evaluate patents which the company may wish to purchase from independent inventors. For these reasons, a clear understanding of the general connotation of the terms, *invention* and *patent*, of the statutory requirements for the protection of possible inventions, and of the scope of current interpretations of patent law, is of the greatest value to all workers and administrators in the field of research. This chapter will attempt to outline the relationship of these factors to research policy. Since no more than a brief sketch of the high lights can be presented here, it is suggested that a more thorough study of patent law will be extremely rewarding to the technician and researcher.

What Is an Invention?

To invent, according to a generally accepted dictionary definition, is to "think up, devise, contrive, figure out, discover by study and experiment." An invention is the "act of inventing or thinking up," or "that which is invented or thought up," or "the act of going through the steps necessary to make an invention patentable." Until we consider the last of these definitions which has reference to patentability, we can see that the product of most research work will be an invention; *i.e.*, when we solve a problem for which we do not have an answer, we "think up, devise, contrive, figure out, discover by study and experiment" an answer which satisfies us. In this sense, we could substitute the word invention for either solution or answer, meaning that inventions are the researcher's everyday stock in trade. If *all* such answers discovered as solutions to problems were considered patentable and if there were no further specifications imposed by statutory or common law, clearly no rights of great value could accrue to the holders of patents granted on these terms. In such a case, the same answer might be patented innumerable times. Under the Constitution the privilege of establishing a definition which would provide both suitable restrictions and incentives devolves upon the Congress. It has been the task of the courts to interpret these definitions in the light

of specific cases and of the United States Patent Office (in the Department of Commerce) to grant patents on inventions which *appear* to fall within the law. These tasks are not simple either in theory or in practice, and therefore patent law remains the complex matter that it is. The courts must finally determine whether an invention is validly patentable under the law; therefore, the patent attorney is an indispensable part of the research process if patents are to be obtained upon the results of the work accomplished. As Tuska points out,

In view of the difficulties, the burden of determining whether or not an invention has been made should be shifted to the patent attorneys. If the patent attorneys have reasonable doubt, they can file a patent application, and thus let the Commissioner of Patents decide. If the Commissioner refuses to grant a patent on the application, an appeal can be taken to the Courts, who in the final analysis determine if a patent should be issued or if an issued patent is valid.³

On the other hand, it is desirable that the research worker understand the background of the difficulties if his work is to have a greater probability of being accepted by the Patent Office and the courts as patentable. We find, therefore, that the meaning of "patentable invention" is of considerably greater importance than "invention" alone, and we shall now examine the former at some length.

The Meaning of Patentable Invention

The foundation of present patent law was laid in 1836, when what amounts to the basic law as it exists today was passed by Congress. This law did not define invention, nor does the present statute (as we shall see), but it did add to the definition above restrictions as to novelty, utility, and limits of time for valid applications. Reading this statute below, we shall not find an answer to the question most frequently asked by researchers, "Is this particular work patentable?"

Any person who has invented or discovered any new and useful art, machine, manufacture, or composition of matter, or any new and useful improvements thereof, or who has invented or discovered and asex-

³ C. D. Tuska, *Patent Notes for Engineers* (Princeton: Radio Corporation of America, 1947), p. 6.

ually reproduced any distinct and new variety of plant, other than a tuber-propagated plant, not known or used by others in this country, before his invention or discovery thereof, and not patented or described in any printed publication in this or any foreign country, before his invention or discovery thereof or more than one year prior to his application, and not in public use or sale in this country for more than one year prior to his application, unless the same is proved to have been abandoned, may upon payment of the fees required by law, and other due proceeding had, obtain a patent therefor. (Section 4886, *Revised Statutes*.)

The important features of this statute may be summarized as follows:

1. Only persons (not corporations) may make a patentable invention.
2. The invention must be new.
3. The invention must be useful.
4. The invention must not have been known or used by others in the United States before this particular discovery.
5. The invention must not have been patented or described in any publication anywhere before this discovery, or more than one year prior to the application.
6. The invention must not have been in public use or sale for more than one year prior to the application.
7. The invention must not have been proved abandoned.
8. Proper requirements as to filing of applications and payment of fees, etc., must be fulfilled.

It should be clear that corporations cannot apply for patents for themselves, although they can undertake to do so for specific employees, since inventions can be made only by individuals. This is an important consideration in collective research and requires that a decision be made as to who specifically has "discovered" the answer to a particular problem, if a patent is to be obtained on that discovery. The courts have generally held that all members of a joint application must have contributed substantially to the entire invention applied for. In a practical sense, this precludes the filing of applications in more than a few names, since it is not likely that the courts would recognize that 10 persons, say, had jointly "invented" something at a specific time. However, in a research team such as we have been

discussing there is little doubt that all members would contribute *something* to the results achieved. If any one of these could show that he had contributed anything of importance to an invention applied for in the names of others, such an invention would not be valid. For this reason, it is of great importance that the proper choice of applicants be made for patents arising from the results of collective research. The research administrators should assist the patent attorneys in determining which members of a given group contributed most to the specific invention which is to be patented.

The concept of novelty is not at all defined by law, and only inadequately so by the courts. Generally, the tests which have evolved are negative ones, *i.e.*, indicating what is *not* new and novel. As the law has been developed, it appears that these tests have become more and more rigorous. The final test of a patent is a Supreme Court decision, and the percentage of cases in which the patent at issue was held invalid has been increasing rapidly, as shown in Table X.

TABLE X.* DECISIONS BY THE UNITED STATES SUPREME COURT IN PATENT CASES, 1900-1945

| Period | Cases heard | Held valid | Held invalid | Held not infringed |
|-----------|-------------|------------|--------------|--------------------|
| 1900-1905 | 9 | 2 | 3 | 4 |
| 1906-1910 | 7 | 3 | 1 | 3 |
| 1911-1915 | 4 | 4 | 0 | 0 |
| 1916-1920 | 16 | 5 | 9 | 2 |
| 1921-1925 | 14 | 3 | 8 | 3 |
| 1926-1930 | 12 | 3 | 5 | 4 |
| 1931-1935 | 14 | 3 | 11 | 0 |
| 1936-1940 | 15 | 0 | 13 | 2 |
| 1941-1945 | 18 | 2 | | |

* J. H. Byers, "What's Wrong with the Patent System?" *Product Engineering*, Vol. 20 (September, 1949).

Any other implications of these facts notwithstanding, this means to the research administrator that, if his company wishes to obtain patents upon his work, he must do everything within

his power to make them stronger and thereby improve their chances of validity. It should not be forgotten that those patents which are brought before the Supreme Court are ordinarily those in which some doubt of validity exists. The stronger they are to begin with, the less likely they are to be challenged by infringement, and the more likely they are to stand up against competing patents. Therefore, we may profitably outline some of the negative tests established by the courts (remembering that further study in greater detail will prove most worth while to the researcher), as discussed by Tuska. These negative rules do not tell the whole story, since they are apt to be subject to exceptions, but they are indicative of the general area of patentable novelty which is available to the research group.

1. The exercise of ordinary engineering skill does not involve invention.

2. The substitution of superior material, which is not new, for the inferior material previously employed, is not invention.

3. Mere enlargement is not invention.

4. Mere change in form produced by mechanical division is not invention.

5. Mere changes in form, proportions, degree, or arrangement do not involve invention, especially when no new principles or no new functions are involved.

6. Unification or multiplication ordinarily involves no more than the exercise of mere mechanical skill, and hence is not invention.

7. Ordinarily no invention is involved in converting from a manual or hand operation to a mechanical operation if there has been no substantial change in the mechanics or method of making the product.

8. No invention resides in adding means to make a device movable when, without such means, the device would not be movable.

9. Omission of parts and their attendant functions, unless the omission causes a new mode of operation of the parts retained, is not invention.

10. Duplication of parts, unless the duplication causes a new mode of operation, or produces a new unitary result, is not invention.

11. Substituting a part for an equivalent part of a machine, process, manufacture, or composition of matter, is not invention unless the substituted part not only performs the function of the part for which it was substituted but also performs another function by another mode of operation.

12. Changes in proportions of a device or machine or manufacture will seldom amount to invention, but it may be invention to change the

proportions of the ingredients of a chemical combination or other composition of matter.

13. The application of an old process or machine to an analogous subject, with no change in the manner of application, and no new result substantially distinct in its nature, will not sustain a patent, even if the new form of result has not before been contemplated.

14. A mere aggregation of elements or a mere aggregation of separate results is not invention.⁴

If all these general concepts preclude an invention from being validly patentable, the research worker then must indeed ask himself, "What is patentable?" If the patents which have been and are being issued are examined, many will be found which appear to run counter to these tests. Yet some of these would no doubt be found valid if they were to be questioned in the courts. The distinctions are subtle, and the best that the research director can do with regard to novelty is make sure that his workers understand these general concepts and have a thorough background in the patents relating to the particular field of their work. Such knowledge and background will prove invaluable in (1) assisting the attorneys in writing the best possible application, (2) overcoming the objections as to nonpatentability over prior art which will undoubtedly come from the Patent Office when the application is filed, and (3) making the strongest possible case in a given instance if the patent is tested in the courts.

An invention, to be patentable, must be not only new but useful as well. For the usual type of industrial research, this is not a particularly important concept, since any solution to a practical problem is ordinarily useful. It is not likely that much work would be done unless it filled a need or was regarded as being valuable—sufficient tests of utility. Of course, utility will not exist if the function performed by the invention is injurious to the morals or health of a community. However, if an invention has both useful and injurious functions, a valid patent may issue upon the basis of the noninjurious function.

Whether an invention was known or used in the United States prior to a particular inventor's discovery may sometimes be difficult to ascertain. The same applies to a prior patent or descrip-

⁴ Tuska, *op. cit.*, Chap. II.

tion in a publication in this or a foreign country prior to the discovery, or more than 1 year prior to the application. However, if a careful search of the literature and foreign patents in the general field has been made, it is hardly necessary for the research worker or administrator to concern himself further with this restriction. On the other hand, for this and other reasons, it is wise for the records of the research organization to be kept in a manner which will aid in establishing the earliest legally satisfactory date of conception of the discovery.

The law with regard to public use or sale concerns itself with the date of the application for the patent on the invention—as does the additional restriction on knowledge, use, or publication—and not with the date of the discovery itself. Public use or sale is by contrast to experimental use to determine the workability or other aspects of the invention. The test of public use is ordinarily the intent, combined with the nature of the particular use. We shall not attempt to discuss the legal considerations connected with public vs. experimental use, but merely point out that it is essential that the research administrator keep his company's patent attorneys fully conversant, prior to an application, with the details of an actual or projected use of the results of any research work which appears worth patenting.

Abandonment of an invention or discovery is not ordinarily a matter of concern for research personnel, since it usually results from deliberate acts of the inventor. Abandonment may result from lack of diligence in applying for a patent. If the research department has placed the proper records before the patent attorneys and indicated the desirability of obtaining patents, lack of diligence cannot be said to be its responsibility. A long delay between invention and application, where the delay is due to the desire to keep the invention secret for the purpose of profit, is also abandonment, but such action is hardly likely to be accidental. Diligence in prosecuting an application, once filed, and actual or statutory abandonment are problems properly left in the hands of competent patent attorneys. If the researcher wishes to learn more of the details of this and other legal aspects of patents, he is referred to the references for this chapter. The use of the background information here can aid the research organization in increasing the *profitable* protection which patents may offer the company. If the research department is to be able

to assist in protecting its inventions so that the strongest possible patents may issue, its personnel must have some knowledge of the mechanics of patents and patenting.

The Mechanics of Patents and Patenting

Thus far we have been discussing the nature of invention and its standing in law. Before turning to means whereby research personnel can protect a company, we shall briefly outline the mechanics of applying for and obtaining a patent. Although the preparation and prosecution of patent applications are the responsibility of the patent attorney, he will need the assistance of the inventor or inventors at each stage of the process. It is his job to translate what the inventor thinks is his invention into documents which, if allowed to issue by the Patent Office, will constitute the best obtainable patent upon that particular invention. The research department should furnish the attorney all the information possible relating to the background of the particular invention, including a history of the art, prior patents, a description of the invention by the inventor or inventors, sketches and drawings relating to the work done, and photographs of experimental models or samples of experimental products.

The attorney may decide to have a search made of prior patents on the subject before writing the application. The patents which are found as a result of this search should be studied by the inventor, and where anticipation appears to exist, he should inform the attorney how his discovery differs from what is disclosed in the particular patents. If it is decided to proceed with the application, the attorney will write a *specification* and have the necessary drawings made. The specification usually consists of (1) a brief description of the invention's particular field, (2) the objects which this particular invention purports to achieve, (3) a list and brief description of the figures of the drawings, if any, (4) a detailed description of the invention, referring to the parts of the drawing, if any, (5) statements covering alternate methods of achieving the invention, (6) a description of the principal advantages of the invention over prior art, and (7) one or more numbered paragraphs constituting the *claims* of the inventor to this particular invention,

limiting his request for a patent to just those things stated therein. Where drawings are required, they will be made by a patent draftsman in accordance with certain definite rules of the Patent Office. The writing of claims is an extremely technical matter in which the patent attorney attempts to define as large a territory of invention as he thinks the discovery warrants. He will usually write some claims broadly, claiming a great deal of territory, and others more specifically, so as to have a second string to his bow if the broad claims are disallowed or found invalid.

When complete, the specification and drawings are sent to the Patent Office with (1) a petition (a formal request for the granting of the patent to the Commissioner of Patents from the inventor or inventors), (2) an oath (the swearing to the truth of the statements in the application and the inventor's belief that he is the true inventor, etc.), (3) power of attorney, and (4) filing fee. If the application is complete in every respect, the Patent Office will give it a receipt and a filing date and so notify the attorney. Following this, in course, will come various actions by the Patent Examiner in the Patent Office, ordinarily pointing out why the particular invention is not patentable, citing prior patents, possibly requiring the application to be divided into two applications because it covers more than one invention, or perhaps declaring an interference with another application covering the same invention, etc. The attorney usually would study each of these actions and the patents cited, and in collaboration with the inventor, make an amendment or response to the arguments of the examiner, if it is decided to continue with the prosecution of the application. In the case of an interference declaration, which will be considered in more detail later, the order of precedence of invention must be proved, and the burden of proof is left to the inventor who filed his application last. If the application is not rejected in an interference, and when the examiner and the patent attorney agree as to claims to be allowed, the Patent Office sends the applicant a Notice of Allowance. The examiner may reject a given claim three times. If the applicant does not wish to appeal to the Board of Customs and Patent Appeals, the third rejection is final. At this stage the patent is ready to issue, and if the final fee is paid within 6 months, the patent is issued on a suitable date after this pay-

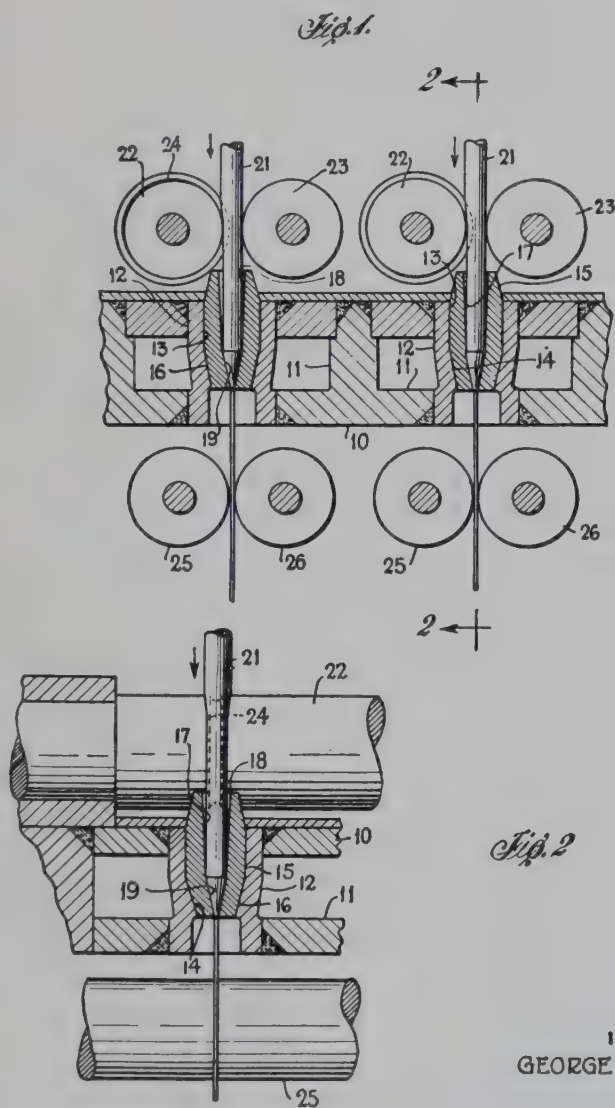
July 19, 1949.

G. A. SPENCER

2,476,830

METHOD OF FORMING FINE FILAMENTS

Filed Dec. 5, 1946

INVENTOR
GEORGE A. SPENCER

BY

Ely & Frye
ATTORNEYS

FIG. 28. Reproduction of a complete United States patent.

Patented July 19, 1949

2,476,830

UNITED STATES PATENT OFFICE

2,476,830

METHOD OF FORMING FINE FILAMENTS

George A. Spencer, Akron, Ohio, assignor to The Firestone Tire & Rubber Company, Akron, Ohio, a corporation of Ohio

Application December 5, 1946, Serial No. 714,188

2 Claims. (Cl. 18—54)

1

This invention relates to the production of fine filaments and other articles from synthetic materials, especially to the production of small continuous articles by a drawing, or mechanical working operation.

Heretofore various types of methods have been proposed for the production of fine filaments from synthetic resinous materials. Most of such filament-forming methods comprise the general actions of heating the resins to plasticize them and give them sufficient fluidity so that they can be extruded through small die apertures. Thereafter, such filaments usually are cooled and then stretched to produce a strong filament of the desired thread diameter. It has been difficult to form threads or filaments of uniform diameters by these previously practiced methods. Also, the synthetic resins tend to break down chemically when rendered sufficiently fluid for extrusion through small orifices since such fluidity is dependent upon the material being raised to a high temperature.

The general object of the present invention is to avoid and overcome the difficulties attendant prior methods of filament formation and to provide a positive action filament forming method whereby a uniform diameter filament is obtained.

Another object of the invention is to provide an easily practiced, inexpensive method of forming fine filaments.

Another object of the invention is to provide a filament forming method which uses a minimum of apparatus, all of which is of substantially standard, uncomplicated construction.

A further object of the invention is to form small diameter filaments from rods of thermoplastic material by mechanically working same.

Referring now to the accompanying drawings, wherein:

Fig. 1 is a longitudinal section through apparatus used in practicing the principles of my invention; and

Fig. 2 is a section taken on line 2—2 of Fig. 1.

Now, referring specifically to the details of the construction shown in the drawings, a die holder member 10 is provided that has a plurality of annular chambers 11 formed therein, each of which is positioned around a tubular die holding member 12 that may be permanently engaged with the die holder in any suitable manner. This die holding member 12 has a bore 13 which has a conical section 14 formed therein for receipt and positioning of a die member 15. This die 15 is of tubular contour and has a conical end 16 which is adapted to seat on the conical section 14 of the die holding member 12. Any suitable means (not shown) may be provided to secure the die 15 fixedly in the die holding member, although such external means may not be required inasmuch as the die 15 is snugly received in the die holding member 12.

2

In order to form reduced section articles by passing the articles through the die 15, it is provided with a bore 17 which has an enlarged receiving end 18 and an inwardly tapered shaping or drawing end 19 which tapers inwardly toward the exit end of the die, as shown in Fig. 1 of the drawings. This die 15 is, of course, provided of such size as to perform a desired diameter reduction on any article passing therethrough.

Rods 21, which are formed of a suitable thermoplastic, crystalline, filament forming material, such as polymers of vinylidene chloride and copolymers of large quantities of it with substances such as vinyl chloride or vinyl acetate are processed in the dies 15. One suitable composition for forming the rods 21 is that sold under the name of "Saran" by the Dow Chemical Company. This material comprises from between 5 to 15% vinyl chloride and between 85 to 95% vinylidene chloride. Usually small amounts of suitable plasticizers or other compounding materials are included in the composition being processed. Substantially the same composition as Saran is also sold in fabricated form under the name of "Velon" by The Firestone Tire & Rubber Company. These rods 21 are formed in any desired manner and when received adjacent the die holder 10, may be of a diameter of, say, .010 to .150", and then by being forced through the die 15, the rods 21 are caused to elongate appreciably to take an external diameter at such time of about .005 to .015". However, larger sized articles such as rods, bars, blooms or, possibly, tubing can be similarly processed and reduced by apparatus embodying the principles of the invention, when desired.

The primary feature of the present invention is that pairs of rolls 22 and 23 are provided immediately adjacent the entrance end of each of the dies 15 and that such rolls engage with and grip on the rod 21. Then one or both of the rolls 22 and 23 are driven at a suitable surface speed so that the rod 21 is forced to flow smoothly through the die 15. If desired, the rods 21 may be slightly preheated in any suitable manner, such as by high frequency electrical currents or by infra-red heat lamps, to assume a somewhat plastic condition, so as to be more easily formed or reduced in diameter by the dies. Such warm-up temperature for the rod 21 may be about 125 to 175° F., but must not soften the rod so as to prevent transmittal of compressive forces therethrough. In some instances, it is desirable to form batteries of dies in the die holder 10, or other similar die holders may be provided adjacent die holder 10. Then the rolls 22 and 23 may be cylindrical in shape and extend between several of such die holders. Recesses 24 are provided in the rolls 22 at proper locations to engage with the rods 21 to aid in gripping and reducing the rods by the rolls 22 and 23.

FIG. 28. (Continued.)

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While the accompanying drawings show several aligned sets of rolls 22 and 23, it will be realized that only one pair of such rolls are required for practice of the invention and that such rolls may have one or more rods 21 fed thereto, dependent upon the desired production conditions, and the number of dies to be used.

Sometimes it may be necessary to facilitate or help the rods 21 to flow or be forced through the dies 13. Thus other pairs of rolls 25 and 26 may be provided immediately adjacent the exit or forming end of the dies 13. Such rolls 25 and 26 then are driven at a suitable speed by any desired means so that their peripheral speed is sufficiently greater than that of the rolls 22 and 23 to compensate for the greatly increased length of the rod 21 due to its elongation by one of the dies 15.

In cold forming the material of the invention into fine filaments, the material normally will be supplied to the dies in an amorphous (and usually supercooled) condition, but the cold working and stretching of such material will effect crystallization of the material so as to cause the crystals formed to extend parallel to the axis of the material being processed. This effects an "orientation" of the material and greatly increases its tensile strength. When required, a suitable lubricant may be applied to the dies and a coolant may be circulated around them in a conventional manner. The dies 15 must be made from a material which is not reactive with the material being processed, and nickel is one material that is suitable for use with Saran.

As a modification of the invention, it may be desirable to pass the rods 21 through a plurality of sets of rolls and dies to reduce the rod diameter in a series of rolling and drawing operations. The rods in such cases then may require an intermediate processing, such as being heated to a temperature at least near their fusing temperature between successive drawing operations, and thereafter they may be quenched to make the material forming the rods amorphous. As the rolls 22 and 23 must engage with the rods 21 to push them through the dies, such rolls must in turn compress the rods 21 slightly and effect partial reduction in their diameters. Normally the majority of the reduction in diameter of the rods 21 at each "roll stand" is caused by the forming action of the dies 15.

Sometimes it may be desirable to omit the dies 15 and use only pairs of rolls as the rolls 22 and 23, to work and reduce the diameter of the thermoplastic rods mechanically, since such reduction will form a smooth, uniform diameter filament. In such instances, it may be desirable to form grooves, like the grooves 24 in each roll, rather than only in one roll, as shown.

Yet another possibility is that the rolls 22 and 23 be omitted, and the rods 21 would be drawn through the dies 15 by the rolls 25 and 26. This action, of course, as could all of the process of the invention, could be performed in a plurality of stages with a preliminary heating action and intermediate annealing and quenching steps, as desired. Also, the rolls 22 and 23 in this case may be grooved like the rolls 22 to facilitate their engagement with the rods 21 and to prevent the rolls from flattening the rod or other article being processed.

From the foregoing, it will be seen that several positive filament forming and article reducing and elongating methods have been provided by the invention and that uniform diameter filaments

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and articles can readily be obtained by practice of the invention.

The invention is applicable to and usable with, in general, the resinous crystalline, orientable polymers and copolymers of vinylidene chloride or 2,3-dichlorobutadiene-1,3. Relatively large quantities of such materials can be copolymerized with minor proportions of other unsaturated compounds such as the vinyl esters on the order of vinyl chloride, vinyl bromide, vinyl acetate, vinyl butyrate, vinyl stearate and the like; vinyl ethers and ketones, such as vinyl isobutyl ether and vinyl ethyl ketone; vinylidene chloride (for dichlorobutadiene); cyclic substituted unsaturated compounds such as styrene, indene, coumarone and the like; conjugated unsaturated compounds such as butadiene, isoprene and the like; and other compounds such as acrylonitrile, and the like. Dichlorobutadiene should be polymerized in the presence of modifiers such as 1 or 2% of amylmercaptan to avoid obtaining an insoluble and infusible product.

In accordance with the patent statutes, one complete embodiment for practicing the principles of the invention has been described in detail herein; however, it will be seen that the scope of the invention is not limited to the example set forth herein, but that it is defined in the appended claims.

What is claimed is:

1. That method of forming fine filaments from resins selected from the group consisting of crystalline thermoplastic polymers and copolymers of vinylidene chloride comprising the steps of forming a continuous rod of such resin, said rod being in an amorphous, supercooled condition, heating said rod to plasticize it, gripping said rod at its sides in the nip of a pair of rolls and revolving said rolls so as to force the rod toward and through an opening of diameter less than the diameter of said rod to form a filament of uniform diameter therefrom, and aiding in the reduction of the rod by drawing it through such opening.

2. That method of forming fine filaments from crystalline thermoplastic copolymers of from 5 to 15% of vinyl chloride with from 95 to 85% of vinylidene chloride, comprising the steps of forming a continuous rod of such resin, such rod being in an amorphous supercooled condition, heating said rod to from 125° F. to 175° F. to plasticize the same, gripping said rod at its sides in the nip of a pair of rolls and revolving said rolls so as to force said rod toward and through an opening of diameter less than the diameter of said rod to form a filament of uniform diameter therefrom, and aiding in the reduction of the rod by drawing it through such opening.

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The following references are of record in the file of this patent:

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| Number | Country | Date |
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| 225,959 | Switzerland | June 1, 1943 |

FIG. 28. (Continued.)

ment, and the company's rights under this particular patent begin. It should be obvious that there are many technical matters which have not been touched upon; only a brief picture of the general manner in which a patent is applied for and issued is intended. Figure 28 is a reproduction of a complete patent as issued, with drawing, in which the various elements described may be noted.

Protecting the Results of Research

Protection of the achievements of the research organization so that patent attorneys may utilize the results of problem solution to obtain the best patents possible is the definite responsibility of all research personnel. Our outline of what constitutes patentable invention and the mechanics of the patenting process in the United States, although brief, should have indicated the importance of (1) dates, (2) diligence, and (3) complete information. Protection, from the standpoint of the research organization, will comprise being *legally* able to establish these items and being able to provide complete details of each to the patent attorney. In any organization, being able to provide required information is ordinarily considered equivalent to being able to produce written records. The more complete these records are, the more reliable will be the information. However, the mere maintenance of complete records may not satisfy the requirement of legal proof. The research administrator should be familiar with methods which will ensure that his records are not only complete but legally acceptable in the courts. It is highly advisable that the practices of a company's research department in this respect be thoroughly analyzed periodically in cooperation with its patent attorneys.

It should be apparent that no one can determine in advance just when work will be done which will lead to important potential patents. In other words, it is not possible to say which of many proposed solutions or avenues of attack for a given problem will be *the* important answer. For this reason, if for no other, it is essential that running records of all activities of the research group be kept by all the workers. Sketches and written descriptions of everything done should be kept. A good method of ensuring that such records are available is to keep them in

bound laboratory notebooks, provided for the purpose. If the results of the day-to-day work are needed in the coordination of the activity by supervisors, duplicate pages with carbons may be provided allowing for the removal of one copy, the other to remain in the workbook. Merely keeping such records is insufficient. They must be signed by the person or persons doing the work, and they *must be dated*. At some subsequent time, they should be witnessed and dated by individuals competent to understand what was done. The nature of the work should be explained to these individuals, so that, if at some later time they are called upon to testify, they will be able to state that they understood the invention or discovery in question on a given date. If these precautions are carried out and the workbooks preserved, they will then serve as *legal* evidence that the work described was performed upon the dates noted. The preservation of the workbooks may be left in the hands of the patent attorneys, who may wish to examine them from time to time. If this is done, the attorneys may be in a position to point out work which is important from a patent standpoint, since they should be familiar with the field of the company's activities and the patent situation therein. In any case, care should be taken to preserve these books against loss. An alternative method, or one which may be used in conjunction with the maintenance of workbooks, is one in which separate documents are drawn up by the researchers and the attorney for *each* conception of an idea, including sketches and explanations, dated, witnessed, and sworn to. Such documents would then be kept by the attorney. Whatever the method, we see that what we are doing is establishing in a legal manner the *first* important date in obtaining a patent—the date of the initial discovery. If the research department is conscientious in this regard and cooperates actively with the patent attorney, we may say that it has fulfilled its primary obligation in patent matters, which is to provide the maximum protection for its work from *its inception*.

Once the conception of an invention is established by proper records, it is necessary that whatever work continues on the particular job also be recorded and the records maintained in a similar manner. Any work done represents diligence on the part of the research department during the so-called "critical" period. This critical period occurs during the time between the concep-

tion of an invention and its reduction to practice. There are two types of reduction to practice, both in use in industrial research: actual and constructive. *Actual* reduction to practice involves the physical embodiment of the invention and its successful demonstration to competent witnesses. The results of such demonstrations should be *written, witnessed, and dated* so that the testimony of the witnesses, if required, can be corroborated legally. The necessity for records of tests and demonstrations is often overlooked in research laboratories, and if the actual reduction to practice precedes the filing of an application for patent (as is sometimes the case in development work), the date of successful demonstrations may be a matter of considerable importance. *Constructive* reduction to practice involves only the filing of a patent application and is held by the courts to be equivalent to an actual reduction to practice. The establishment of this date of application is ordinarily the responsibility of the patent attorney, and it remains for the research organization merely to give him all the cooperation he requires in writing a satisfactory application.

The best time for filing is subject to a number of other factors, having to do with applications of the company already on file in the Patent Office, the competitive situation, etc., and should ordinarily be left to the judgment of the patent attorney. In any event, the research group will usually fulfill its responsibility by maintaining suitable records. The question of diligence may not be important unless the field of the invention is an active one and another inventor enters it after the first conception. Of course, to assume that a particular group has the field to itself may be taking a serious risk, and decisions in such matters should not be undertaken by the research department alone.

If the second inventor conceives the invention and reduces it to practice before the first inventor has perfected the invention, the first inventor will lose his right to the patent if he was not diligently trying to reduce the invention to practice from a period just before the second inventor entered the field and continuing to the reduction to practice, or if he can [not] offer a reasonable excuse for his lack of diligence.⁵

We have already mentioned the possibility of a patent application being declared in *interference* with other applications in

⁵ *Ibid.*, p. 78.

the Patent Office. Since a research department is often involved in such proceedings, which are instituted to establish the right of one particular inventor to prosecute an invention, a brief outline of the procedure is in order. Again, we state that no attempt will be made to cover the complete legal story. Information in this regard is left to other sources, and action taken is the responsibility of the patent attorneys. An interference action may involve any number of applications which in the eyes of the examiner cover the same material. In an interference proceeding, common claims must be written into all the applications involved, and this is done on order of the Patent Examiner by the rival attorneys. After this is done, each party is required to file a "preliminary" statement under oath, giving the date of conception of the invention, the dates of the first drawing and written description, the first disclosure to others, the date when reduction to practice in the United States was completed, the date when the inventor began perfecting the invention, etc. Once this is filed, no changes can be made in the dates, so we see again the importance of having adequate dated and witnessed records. If the date which the junior applicant swears is the date of invention is not prior to the filing date of the senior party, the senior applicant is given the rights of the invention, and no interference is declared. When the statement is filed and a junior party claims an invention date prior to that of the senior party, the various parties to the interference are permitted to see those parts of their opponent's application which are relevant to the interference. There then elapses a period during which the various parties to the suit may bring various legal motions to dissolve the interference, shift the burden of proof, etc. Hearings are held on such motions in the Patent Office, and they are ruled upon by the examiner. If the interference continues after the motion period, sworn testimony is taken in which each of the parties attempts to establish the dates outlined in his preliminary statement by producing evidence, examining research personnel and witnesses to the work done, etc. This testimony is taken first for the last party to file his application. Thus, the senior party (or first to file) is in possession of the testimony of his adversary or adversaries before he is required to present his witnesses and testimony. All such testimony is subject to cross-examination. The burden of proof is on

the junior party, and without the assistance of adequate records, it is seldom possible for research personnel to establish satisfactory dates. There are numerous legal alternatives in interference proceedings for details of which the reader should consult the references. The research worker or administrator should, however, always keep in mind, as he does his work of problem solving and prepares his records, that he may be called upon to testify in such a proceeding. If he is to be prepared to have his *idea* of the date of conceiving a particular invention stand up under vigorous cross-examination, he must rely upon the written records of his laboratory.

The outcome of an interference depends largely upon the parties' dates of conception and reduction to practice. One inventor usually conceives his invention before the other does. If they both proceeded with reasonable diligence in reduction to practice or in filing their applications, the inventor who was the first to conceive would be awarded the patent. Conception takes place when the inventor visualizes in his own mind a structure or process whereby the result sought can be accomplished although this need not necessarily be what the inventor intends to build for commercial use.⁶

Infringement is another matter with which the research organization may sometimes be concerned. It comprises the use, in a broad sense, of the art which a particular patent teaches without the consent of the owner of that patent. In a particular company, the importance of infringement may have to do either with others infringing its patents or with its infringement of the patents of others, or both. In any case, the decision as to whether or not infringement is being practiced and what to do about it is generally not a matter for the research department to determine. However, the technical aspects and means of proving that a patent is being infringed in the first instance, or not in the second, are certainly matters to be considered by research personnel in cooperation with patent attorneys. Indeed, a knowledge of the patent situation in a given field is necessary if the research administrators are not to lead their organizations to develop inventions which may be useless, or represent wasted effort because strong prior patents exist. In like manner, the

⁶ A. K. Berle and L. S. DeCamp, *Inventions and Their Management*, 2d ed. (Scranton, Pa.: International Textbook Company, 1947), p. 324.

production or sale of certain products by competitors may not be possible except by methods which infringe patents of a particular company. The research department must be awake to its responsibility for determining where such cases exist. In an industry dominated by patents, infringement suits are not uncommon, and the testimony of research personnel in these suits may be most important in the determination of the outcome. As an example we may cite the radio industry where 1,567 infringement suits were brought between 1900 and 1942, involving 684 patents. Seventeen per cent of the patents involved were declared valid and infringed, 3 per cent valid but not infringed, and 3 per cent invalid.⁷ In most of these cases the testimony of the inventors (and other experts) was the determining factor in establishing infringement and/or validity. Therefore research personnel must be prepared to defend their inventions against counterclaims and to protect the interests of their companies in proving that they are not using what belongs to others in carrying out their business. The decisions to engage in legal action and the determination of the manner in which suits are carried out must, of course, be left to others.

Ownership of Patents

We have pointed out that patents may be issued only to individuals and not to corporations. Then how does a corporation come into the possession of its patents? A patent is ordinarily the exclusive property of the inventor. It may be treated as any other piece of property, to be sold, "rented," given away, etc. Therefore a company may buy or license patents from independent inventors or from other companies. In the case of research organizations, which are set up to produce what amounts to inventions, the purchase or license of each such discovery is not desirable. Nor is it particularly expedient to rely upon the doctrine of shop rights, which gives to the employer the right to use such inventions as have been developed with his materials on his time. Such rights do not extend beyond the employer's

⁷ W. R. Maclaurin, *Invention and Innovation in the Radio Industry* (New York: The Macmillan Company, 1949), p. 273. Some 33 per cent were dismissed or settled out of court and 32 per cent involved a consent degree, validity not being determined.

business, and ordinarily this is not enough. For this reason, the prospective employee of an industrial research organization is usually required to enter into an oral or written agreement in which he assigns to his employer such inventions as are made during the course of his employment and relating to his employer's business. In addition, such agreements usually specify that the employee will not violate his employer's confidence regarding the particular business during his employment (and afterward, where compensation is provided therefor). These agreements are usually written by the company's attorneys, and the exact manner of their preparation should be left to them.

The consideration for which such an agreement is accepted on the part of the employee is usually a stipulated salary or wage. However, additional payments for patents issued may be specified, as well as consideration for signing applications for patents in the United States and foreign countries. We have earlier discussed compensation for research personnel and have pointed out the dangers of attempting to create an incentive system for research personnel based upon patent applications. We have pointed out that this could lead to jealousy, internal friction, and secretiveness, all of which would tend to lower the efficiency and morale of a research organization. Where non-technical personnel, not covered by agreement, make patentable suggestions, the company may request that an assignment of rights to the patents be made upon the payment of compensation. Otherwise only shop rights may be claimed. There are companies who pay special bonuses for exceptional inventions, but the general consensus among research executives is that, except for nominal fees for applications, etc., the rewards to research personnel should be in the form of promotion and increased salaries. It is not likely that research personnel who are paid more than they could expect elsewhere and who work under stimulating and interesting conditions will be dissatisfied because they do not receive special compensation for patents resulting from the nature of their work.

Where patents or ideas are purchased from outside organizations or independent inventors, the matter of compensation is for management determination. However, the research department should examine the patent and give its opinion as to the technical merits of the invention claimed. It should be appar-

ent that the patent attorneys will be called upon for an opinion as to the legal merits of the particular patent. Usually some form of protection to an outside inventor is provided where a patent has not been issued. Some companies refuse to consider an invention unless the inventor has already filed a patent application. In any case, the exact price to be paid should be determined upon the basis of all the information available as to the value of the invention to the purchaser. This should include (just as the analysis of a research proposal) technical, market, sales, production, and legal evaluations.

Where purchases of patents are not expedient or possible, some form of royalty licensing arrangement may be made. These provide for the privilege of practicing the patented invention in consideration of certain payments, with the patent owner retaining title to the patent. Types of payment vary from licenses per unit (so much per ton, piece, etc.) to periodic (so much per year, etc.) licenses. The agreements may be completely exclusive, or partly so, or nonexclusive, depending upon the circumstances. As a rule, the research director or administrators are not made responsible for negotiating such agreements. They will be called upon for technical opinions as to merits and future possibilities which may aid top management in deciding (1) whether to take out a license under given patents, (2) what these licenses are worth, and (3) what policy of compensation offers the best advantages. In general, the patents of most industrial corporations are available for licensing under proper agreements, and certainly the independent inventor is anxious to obtain a use and receive compensation for his inventions.

Another method for profitable use of patented inventions on the part of an individual company is to engage with others in a *patent pool*. This usually comprises a group of patents relating to a particular field which are placed in a common group by their individual owners so as to allow for their combined use by the owners and others. Patent pools used for the purpose of restricting competition are open to prosecution under the anti-trust laws. However, where these pools are open to anyone under a common scheme of licensing, they can be of great benefit to industry and the public in making for higher productive efficiency. The creation of patent pools and their management

are of great interest to research personnel, since, by and large, the efficiency and worth of any given group of patents can be determined only technically. Therefore, we often find research directors managing such patent pools, or at least being called upon for technical advice as to their management. Patent pools should be established upon the basis of making a wider range of the specific art open to their members and licensees, and the question of joining or licensing from a patent pool should be answered upon the basis of increased quality or decreased cost.

The Evaluation of Patents

All the questions raised thus far as to the compensation for, purchase of, and licensing under patents have left to the research personnel the problem of technical evaluation. Up to this point, we have discussed the background of patentable inventions, the mechanics of obtaining patents, the protection of patentable discoveries, and the ownership of patents, but we have not considered just what it is that a patent comprises. This is perhaps fitting, since the brief preceding background will permit of a better understanding now. A United States patent is the right, granted by the United States government⁸ to an inventor or inventors, in return for a complete disclosure of an invention, to prevent others from using or selling the invention without express permission, for a period of 17 years, at which time all rights in the invention pass to the public at large. It will be seen that a patent does not *necessarily* give the inventor the right to use his own invention, since his may be an improvement on some other individual's patent, and he will not be able to use the whole device or process without permission of the other patentee. However, assuming that he is not infringing upon another's patent, he may use, manufacture, or sell his invention or the products therefrom to his own profit for a period of 17 years. Or, he may enter into such permissive agreements as he sees fit to allow others to use the discovery. However, he is limited to what is described in the claims on his patent.

⁸ Foreign patents have not been discussed, but patent systems in other countries are by no means the same. Problems of foreign patenting are ordinarily not of importance to research personnel and may be left to attorneys who are familiar with the field.

Therefore, a patent represents an area of potential industrial operations which is bounded by the claims *as written in that patent*. If there are "gaps in the fence" which surround this area, or if the patent claims something which has already been invented (the fact that a patent has been granted does not, of course, afford any guarantee as to how a court will find if it is tested), it is certainly less valuable than if it does not have these deficiencies. The researcher will be called upon to analyze patent applications on his own inventions, patents of competitors, patents which may be purchased or licensed, and perhaps to testify in court concerning the validity of his own or others' patents. Hempel has given an excellent approach to such analysis, which is of value to all technical personnel concerned with patents:

1. Read the patent carefully, and while doing so make notes on all points which at the first reading appear to be important, for or against technical or economic suitability.
2. Study the technical content especially, looking for desirable or undesirable features.
3. Study the claims, their validity, feasibility, and what they reveal in reference to applicability, convincingness, etc.
4. Try to discover if other claims could be made, on points not covered, left out, or only incompletely covered.
5. Appraise the economic success probabilities of the patent; first, with a hopeful attitude, and then with a skeptical line of thought.
6. Estimate roughly the complete cost of the equipment, cost of installation, and cost of operation and use; compare with the costs of other methods so far used or known, of achieving the process; and decide tentatively how the new invention compares. This can be done without much investigation, and should be a mere tentative procedure, to be followed by careful investigation, if favorable.
7. Judge the economic need and the advisability of having and using the invention, its general suitability, and useful range of application.
8. Put the patent away for eight or more days and file the notes.
9. Analyze all over again without the previous notes, step by step, putting down the findings on new sheets, or have other people make an analysis.
10. Compare the findings and summarize them, so as to bring out
 - a. the theoretical weak and good points,
 - b. the practical weak and good points.
11. Form a decision.⁹

⁹ E. Hempel, *Top-management Planning* (New York: Harper & Brothers, 1945), pp. 241ff.

It may even be desirable for the research director to undertake laboratory investigation of the claims cited in an invention under consideration, to determine technical feasibility and economic advantages. It is, of course, within the scope of his duties to find ways and means of accomplishing the same purposes without infringing upon competitor's patents. In a highly competitive industry, this will require a close familiarity with the existing patent situation and an understanding of patent law.

As a general rule, a single patent is not sufficient to provide protection to an invention. It is quite rare to find any important industrial product or process covered by only one patent. However, a patent structure erected upon a basic idea can provide the best possible protection under the law. For this reason, research departments are usually charged with the responsibility for "inventing" improvements in the various areas of interest to the business, and of extending the patent coverage to surrounding fields. If an industrial research department works for long periods in a given area of problems, patents should issue from time to time which will enable the company to maintain profitable activities in this field and pay for the research efforts expended for periods extending beyond the original 17 years granted in the first patents. Such extension, improvement, and prolongation of patent coverages give rise to the multiple patent structure, such as is ordinarily the result of continued research activity. Research administrators should be able to analyze and evaluate such structures, both of their own company and of others, in much the same manner as discussed for single patents. Determination of technological strength or weakness in these patent setups will enable a company to pursue an aggressive research policy.

Summary

This general outline of patent policy has been intended to bring out the important relationship of the research administrator and worker with the patent attorney, and to indicate the overwhelming importance of keeping adequate records if the results of research work are to be protected under the law. The difficulty of defining in advance what constitutes a validly patentable invention only points up the necessity of close collaboration between technician and attorney. This is to be accom-

plished by furnishing the latter with as much information concerning the art and the specific invention as possible. If the attorney is to write and prosecute patent applications with vigor, protect them in interferences, and later be able to use the patents obtained in denying to others that which rightfully belongs to the company by virtue of having been discovered in its research laboratories, he must have properly kept and legally witnessed records to rely upon and the full cooperation of research personnel in giving testimony where required.

Research personnel will usually enter into agreements with their companies to assign all rights in their inventions, in return for adequate compensation. Incentive systems based upon patents obtained can do more harm than good, since successful research depends upon teamwork and cooperation, which is endangered by any system fostering secrecy. A compensation system based upon promotion and salary increases is likely to be the most efficient in obtaining adequate research results.

The ability to analyze and evaluate patents and patent applications should be a part of the equipment of all industrial research personnel. In large measure, it is the responsibility of the administrators of research to know themselves, and foster in their workers, the background of the patent system, its limitations, and the technical meaning of patents. In some cases, the director of research may even be responsible for directing the work of patent attorneys in carrying out the patent policy established for a company. In such instances, the necessity for having an understanding of patents is even greater. Whatever the specific organization, much is to be gained if all those engaged in research recognize and use the advantages which will accrue from a knowledge of what patents are, how to analyze them, and how they are obtained.

CHAPTER XIV

EXTERNAL RELATIONSHIPS OF THE RESEARCH DEPARTMENT

Research and Public Relations

The industrial enterprise and its research workers have been increasingly deprived of "simple" problems as the national and international economic systems in which they exist have become more and more complex. The problems which, economically, appear to warrant solution by industry have broadened in scope and have become more difficult to resolve in terms of the resources of all types needed for their solution. In our attempt to synthesize methods for organizing and administering industrial research departments efficiently, we have argued that the broadly constituted research team would be most suitable for solving these problems. From a narrow technical standpoint, this might possibly be sufficient, but in reality the picture is a great deal more complex. All members of the immediate industrial and economic environment have their effect upon the results of research activity. We found in our earlier discussion of the creative mentality that the scientific worker is influenced by all aspects of his total cultural environment. For this reason, it is necessary that we consider the relationships of industrial research organizations with those external influences that most seriously affect their activity.

The most immediate of these influences is, of course, that which stems from those members of the same industrial organization who are not a part of the research group. It has been shown that the important research policy decisions must be made by the top-management personnel of a given company. Good channels of communication between the research administrators and these executives, who will probably include representatives of the sales, production, legal, finance, and other departments, are essential for sound decision making. The necessity for an un-

derstanding of the problems which exist at the working level in all these departments, if research is to be a vital part of the company's activity, has been made quite clear. In addition, there is an opposite side to the relationship: it is equally important that the executives and workers throughout the organization have an understanding and appreciation of what the particular research group is. Perhaps this is not essential for technical operations. If the existing reciprocal influences are to be healthy ones, and if the research department is to receive the active cooperation and support of the other members of the organization, the pattern desired must be *actively* fostered. We shall take the attitude that the onus is upon the research group to maintain and cultivate this mutual understanding. Actually, this is neither necessarily nor completely true, but we wish to study the problem from the research administrator's standpoint. To take the view that the burden lies elsewhere is merely to dispose of the problem without attempting a solution.

The chain of productive economy, which may start in the research laboratory with the solution of a particular problem, continues through the actual production process and thence to the ultimate consumer through various marketing procedures. The flow of information which *must* proceed from the research laboratory in this direction will be paralleled by a feedback of information in the opposite direction. In other words, the consumer's actual or potential desires, translated by those who are in direct contact with him and supplemented by the information available from those supervising production, should eventually reach and influence the researcher in the laboratory. (Although we are specifically concerned with the action of producing goods for sale, with which the greater part of industrial research personnel is connected, this same flow of information holds for the other types of research we have described—market, organizational, etc. There are producers, consumers, and sellers of the results of these types of activity also.)

Every industrial enterprise has some measure of concern for the public opinion in which it is held. The present technological awareness of the people of the United States makes the research department of any company play an increasingly prominent role in commanding a favorable attitude for the entire organization. This is true whether or not the company produces and sells con-

sumer goods. A considerable portion of modern advertising is directed toward displaying the preeminence of research groups to the public. Obviously such display and impression creating cannot be done satisfactorily with the *negative* cooperation of researchers. Suitable good will and feedback of desirable information from both the consuming population and the general public demand a constructive attitude on the part of research organizations toward public relations.

Nor are these all the external relationships which are important in assuring that the research worker becomes an integrated part of the whole environment, as far as the industrial enterprise is concerned. We have already discussed in some detail the necessity of his close cooperation with the company's patent attorneys for the purpose of securing the strongest and broadest protection for his work which may be possible under the law. This is but one of many areas in which he will be brought into contact with agencies of the government. For example, his company may be directly engaged in research for one or more of the various Federal, state, or local departments and agencies which go directly to industry for the solution of specific problems. In such circumstances, he may be working almost directly for a governmental body. In other cases, he may serve upon advisory committees for regional or city planning, for agriculture, for the national defense, etc. Therefore, the research department must be prepared to establish a policy for such relationships and foster whatever measure of cooperation may be desired.

In addition to the members of his own firm, the consuming and general public, and governmental agencies, the researcher will have contacts with other industrial enterprises, both competitive and noncompetitive. Here, the respective research departments may be in direct contact with each other, or the relationship may be with departments of the other companies, such as sales, production, etc. The creation of good will and suitable public relations may be of great importance for the research department in this sphere, leading to broader viewpoints and perhaps helpful assistance in the solution of problems.

Such activities are closely akin to relationships with industrial and trade associations, composed of companies with similar interests. A particular company may be a member of a trade association which has research laboratories of its own and is actu-

ally pursuing work on problems of general interest to the members. The research administrators of the company may be actively concerned in the over-all direction and coordination of the association's research. Such contacts may also be of prime importance in establishing suitable relations with members of the association's research staff, as well as with the other industrial members of the organization. Certainly where trade and industrial associations exist, the research personnel from a company are important emissaries in creating good will and sound feelings with the other members.

The professional societies, of which most of the research workers will be members, serve the research departments of industry in innumerable ways. We have already demonstrated their importance in the matter of cross-fertilization of ideas, as well as in morale, and have indicated that the worker may be stimulated beyond all proportion to the cost if he is encouraged to take part in this type of activity. And, of course, the sheer matter of publication and dissemination of research results of general interest has been undertaken by these societies. The industrial organization can hardly afford to overlook the necessity of creating and maintaining cooperative relationships toward the societies in which its technical workers are interested. The same is true of academic institutions. Industrial research departments, and through them the over-all enterprise, can serve and be served by maintaining suitable contacts with the various universities and colleges.

We can see that, although the research organization might avoid contacts and relationships of a constructive nature in one or two of the areas mentioned, it would not be possible to assume a completely negative attitude toward all. For this reason, we shall examine in somewhat greater detail ways and means of creating policy with regard to these various types of relationships, and methods of implementing those policies. In general, we may say that the better the contacts and relations in all these fields, and the more friendly and complete these relations, the more the industrial research department will stand to gain in the long run. Our attitude in the following discussion is derived from the viewpoint that good public relations are friendly ones and that these are the ones which are most desirable.

Relationships with Other Employees of the Company

What is misunderstood is often resented, in industry as elsewhere. A research department shrouded in mystery is apt to be no exception to this attitude. The research worker, whether he is a professional or a laboratory technician, is often afforded what appear to be special privileges to his clerical, producing, and other fellow workers. His workplace may be exceptionally clean, it may be air-conditioned, they may *think* his wages are better (and they may be), his hours may be different, etc., and unless the reasons for such differences are made clear, they might lead to a general attitude of hostility to research on the part of the other employees. Such an attitude may well be shared by administrators and executives, although they may be more subtle in their reasoning about it and in the manner of showing their resentment. As further fuel for this attitude, the workers may believe that the sole objective of research activity is the development of methods to cut down employment or degrade the skills of those working. Supervisors and executives may believe that it is a function of research to find out and draw attention to areas in which they have not done so suitable a job of directing as might be possible.

Such feelings on the part of the fellow employees in a company toward their researchers can do only harm in the long run, both to the research department and to the entire enterprise. These attitudes may arise through real causes; *i.e.*, the research department may be at fault in adopting the wrong view of the remainder of the company. However, the situation is just as likely to come about through nothing more than ignorance of the reasons for having research, and of the results of research activity in the past. Where it is allowed to develop, it is extremely difficult to eradicate or to keep from growing worse. It is only natural to anticipate that research personnel, if they know they are resented or misunderstood, will adopt a defensive (and perhaps offensive) attitude which will not aid in creating good will and mutual understanding. Therefore, it should be the responsibility of the research administrators to investigate the feelings (and knowledge) of all the other employees of the enterprise toward

the research department, and to undertake a program which will develop satisfactory and friendly relationships at all levels.

The stimulus of a joint understanding of mutual problems and the comprehension on the part of the research worker of what his solution to these problems may mean to the sales or production worker cannot be overemphasized. The value of suggestions for problem solution, or of worth-while problems alone, from workers on the "firing line" to the research department is equally important. Where the sales department, for example, holds the belief that "the research department is continually promising something it cannot or will not deliver," and the research organization feels that "sales is always selling something before we have the problem solved," a great deal of the value of each of these groups to the other and to the company is undoubtedly lost. Hence, a program must be undertaken to prevent such situations from arising, and to correct them if they do exist.

Since we are considering only the research department's responsibilities in this regard, let us first say that the directors of this activity should see to it that the functions and importance of the other departments in the company are presented to, and understood by, their personnel. This is a primary and extremely important step in any program to establish cooperative operations. The researcher should understand the manner in which the results of his work will be used, and what they will mean to the company. (We have already seen that the achievement of tangible results is an important factor in his morale, thus we can obtain a multiplicity of benefits from this step.)

If the research personnel understand and are sympathetic to the other departments of the company, the task of selling the group to the rest of the organization will be made much simpler. It becomes then merely a problem in (1) making the objectives and operations of the department clearly understood, and (2) establishing formal and informal channels of communication whereby the research workers can exchange information with other members of the company, and vice versa.

The specific means for accomplishing each of these objectives are subject, of course, to whatever general policies in these regards have been established by top management. In most cases, these policies would encourage and not hinder any steps that might be taken. However, the task of interpreting research to

the other employees may be placed in the hands of those responsible for labor relations or other employee activities. In any event, the methods used will probably be much the same. In making the objectives and operations of the research department understood, the various house organs, stockholder and employee reports, and other means, should be used to include simple and clear stories about those who are working in the research department, what they do, and what the research department has done in the past to provide *present* jobs for the remainder of the workers. As an example of this type of information, in E. I. duPont de Nemours in 1948, there were 20,000 employees producing or selling products which were not produced commercially in 1936, and jobs had been indirectly created for thousands of others. The importance of the types of research that are being undertaken and the meaning of this department to the long-term future of the enterprise should be set forth. If working conditions in the research laboratories are radically different from those in the rest of the company, the reasons for cleanliness, air conditioning, etc., should be forcefully presented. Wherever examples are available of assistance that *other* departments have furnished in solving research problems, these should be emphasized. The necessity and utility of cooperation, plus the fact that professional personnel in research are just people doing their jobs, are the thoughts which this type of information should leave with the other employees. Concrete exhibits of the type of work being done in the laboratories, and the end results of this work in solving problems which are understandable to the persons to whom the information is directed, are of great value and can be set up in plants remote from any research activity. Where they can be arranged, tours of the research laboratories and explanations of the equipment and work are well worth the effort. It is likely that resentment will be the result where outsiders are shown through the laboratories with great ceremonies, while company employees are barred from visiting them.

In communications on a higher level, the research directors and administrators should lose no opportunity to make known their appreciation of the work of their confreres in sales, production, etc. They should also use their positions to indoctrinate these same people in the methods of research, making sure that they understand what research can do and what it cannot do, and what the results of the research solution to a problem mean

and what they do not mean. They should not in any way consider themselves apart from the remainder of the enterprise but should take an active interest in all the company's problems. They should also take part in reaching decisions where their responsibilities and capabilities call for such action.

If these objectives are actively sought, channels of communication will automatically open up both horizontally and vertically. However, additional steps should be taken to assure that exchanges of information, which are so important from the standpoints of good relations and technical achievement, are not lacking. For example, technical personnel from the research group may be assigned periodically to work in various other departments of the company. Personnel in these other departments who may have been technically trained can be assigned periods of orientation and indoctrination in the research laboratories. This is important in many industries for both production and sales employees. As an example, there are companies in which new production and sales supervisory employees are technical graduates and are automatically given a period of indoctrination in the companies' laboratories or pilot plants. (Pilot plant operations are considered particularly useful training for technical service, market research, or sales people in the chemical industry.) Reports of research activity may be circulated to members of the company who ordinarily would not have the opportunity to see them. In the other direction, operations reports from production or complaints from sales may be circulated to the research laboratories. A traveling team of research personnel can be used profitably to visit production facilities and discuss with the supervisors *and workers* problems of production and of research. Periodic meetings and conferences with all types of personnel can be arranged in the research laboratories. If such meetings are held, it will be found that the range of topics discussed is practically unlimited.

All these means on the part of the research, the labor relations, and other administrators are principally for the purpose of engendering a spirit of mutual understanding. If the research personnel is friendly to the remainder of the company's employees, and they in return exhibit a spirit of understanding for the problems of research, then the problems of cooperation will take care of themselves for the most part. The practical results will be more productive and fruitful research.

Relationships with Other Industrial Companies

The other industrial companies in the economic environment can be divided from the standpoint of a particular research department into three categories: (1) competitors or potential competitors, (2) actual or potential suppliers or consumers, and (3) all other firms. The relationships established will be somewhat different for each of these. We may say, in general, that the minimum objective in all these groups is a friendly and courteous attitude. The emphasis and degree of interrelationships vary in each case.

Competitors will be more closely allied to a given company's research department than might appear to be the case at first glance. They will be subject to much the same public and consumer influences and will probably be faced with similar production and marketing problems. The respective research departments will be composed of similarly educated professional personnel, and in highly specialized fields much of this personnel may have come from the same academic institutions. The researchers will undoubtedly be members of the same professional societies and may have close contact through these agencies. Where the industry has trade associations, competitive research personnel will serve together on the various technical committees and panels.

Much good can come of general friendly cooperation among the various research departments. It can lead to advantages for the general public as well as for the competing companies, in the general areas of safety, standardization, and end-use improvements. This leads directly to a consideration of where the line must be drawn between secrecy and a free exchange of information. Complete secrecy is not warranted in the public interest or, when analyzed, in the interest of the competing firms. Complete cooperation is not permitted under existing antitrust laws. Nor is it certain that the competitive economy is best served by complete exchange of all information. It seems likely that problems of general interest to all competitors, which will improve the standards or decrease the working hazards, should be the subject of mutual cooperation. Further, when research departments obtain data leading to the solution of problems of

a general nature, not of an immediate competitive advantage, the best interests of all may be served by providing for their dissemination. It would appear that the competitive system requires the preservation of certain types of information, if enterprises are to gain profit from their research activities. This information would probably include the details of immediate problems which have been determined as worth research and, as required by the patent laws, the exact manner of the solution to these problems, at least until patent applications have been filed.

As an example of permissible and desirable cooperation, the case of two competitors involved in producing the same product in a highly hazardous process may be cited. The research department of one of these companies had studied the safety problem in great detail and had worked out certain methods which increased the production workers' safety. In cooperation with the other company, these same safety methods were then installed at the latter's plant. The result, while of some *competitive* advantage to the first company, was of far greater *public* benefit when used in both plants. The development of standard tests and nomenclature for the protection of the public, through such industrial associations as the American Society for Testing Materials, may similarly be cited as an example of proper and necessary competitive cooperation. It should be apparent that there is a limit to the information which may be so interchanged. If the exact problems, their economic analyses, and the results of day-to-day research were made available to all competitors in an industry, there would be little value in individual research laboratories. In general, the test should probably be the general or common interest as opposed to specific competitive interest. That knowledge or information which is of general interest can be used for competitive work is freely granted, but in the long run the gains from the exchange of such material will outweigh any short-term losses to competitors.

Thus, free discussion of general technical problems confronting an industry can serve as valuable stimulants to research activity on specific points. If there is a limit to the amount of information that can be exchanged, there is also a limit to the degree of secrecy which is possible if creative personnel are to put forth their best efforts. If a company is working upon a

new process to supplant one used by its competitors, which will produce more efficiently (or with higher quality products), it certainly would not like to have free discussion among its own and competitor's personnel of the research being undertaken. However, once the research is completed and the new process is in operation, the need for such a high degree of secrecy no longer exists. There are companies who demonstrate to the entire industry their newly developed processes as soon as they are producing in their plants. It is their opinion that their research will have placed them several years in advance of their competitors, and showing the finished process will cause them to suffer no losses. These companies feel that, if their competitors imitate what they see, by the time anything has been accomplished, new and different results will be available from the research laboratories to maintain the original advantage. Whether this is true is not important, but the point that such general communication of information may be of advantage to *all* the companies in an industry is of great interest. This same conclusion holds for the publication and patenting of new developments. Our discussion of the methods of protecting the results of research problem solving in connection with patents indicated that the attempt to hold these results without patenting (as trade secrets) did not necessarily afford a high degree of protection. This is worth reconsidering in the present connection, and it should be noted that many large companies have placed their entire holdings of patents on the Register of Patents Available for Licensing or Sale of the Patent Office. This is a matter of legal and public relations in connection with antitrust legislation and prosecution.

The most noteworthy means of cooperating and maintaining a suitable attitude toward competitive research groups is through trade associations, the best of which are those which are open to all the members of an industry. In widely dispersed industries having large numbers of competitive firms, regional associations provide the means for continued contact. Direct discussion with specific competitors on problems such as have been mentioned is possible but less advantageous to the industry as a whole than the use of a trade association. Such associations may establish their own research organizations to work on problems of a broad scope, which might be too complex or insufficiently com-

petitive for member firms to handle. Thus, the American Meat Institute, composed of producers in the meat-processing industry, established a research laboratory in 1925. It now has a research staff of 31 persons, who are working on "fundamental studies in curing of meat, prevention of spoilage, manufacture of lard, and chemistry of fats, nutritive value of animal by-product protein feeds, amino acid composition of meat." Another institution, established through grants from large textile companies in 1930, is the Textile Research Institute at Princeton, New Jersey, which is working on "fundamental chemical and physical studies of textile fibers, yarns, and fabrics and long-range engineering research of textile products and processes."¹ There are a great number of these associations and many of them either engage in or sponsor research at universities and institutes. In cooperating with these groups, the individual firm should lend all technical assistance possible, make time available to its own research personnel to attend meetings and conferences, allow the members of the association to inspect such facilities as may not be considered of direct competitive value, see that fundamental work of its own researchers gets published, and limit its policy of secrecy to only those areas where it is necessary. The benefits to industry from such cooperation with competitors have been summarized by Larson as follows:

1. Trade association research offers a way for small manufacturers to tackle industry-wide problems that are too large for individual manufacturers; also, association research offers a method of providing some research to small manufacturers rather than none at all.

2. Trade association research can be done at a minimum cost because duplicating research efforts are eliminated.

3. Trade association research contributes to reductions in manufacturing costs and enables lower prices to consumers.

4. Trade association research improves materials, processes, and products, resulting in better products for consumers.

5. Trade association research expands markets and protects new markets through improvement of present products and development of new products.

6. Trade association research offers a way to obtain research results of related industries and government agencies at a minimum cost.

¹ G. E. Larson, *Trade Association Industrial Research* (Washington: U.S. Government Printing Office, 1948), p. 3.

7. Trade association research has advertising and prestige value in that it gives a benefit to consumers which can be talked about.²

In dealing with industrial firms which are either actual or potential consumers of a particular company's products, or suppliers of raw materials, the research department will find a co-operative attitude essential. If the group or its administrator does not have such an attitude to begin with, it will in all likelihood be forced to adopt one by the pressure of management or other departments. The necessity for understanding the problems of the consumers, for working with them, and for teaching them to use the company's products to better advantage has already been discussed. To know these problems and what the industrial consumer needs or is likely to want is the first requisite of any research group working on problems of product development. Similarly, to work cooperatively with the suppliers of the raw materials used is essential if improvements are to be made in both products and processes.

This type of cooperation is achieved through direct contact, through technical service organizations, and through representatives in other departments, such as purchasing and sales. The best method of indicating to the consumer or supplier that the research department is anxious to cooperate is for individual research administrators and workers to take a direct interest in their problems. In some cases, these problems may be brought into the laboratory and attempts made to solve them there. When complaints and problems are brought to the attention of the research group by the sales department, constructive answers should be supplied. Any attempt to ignore them or to show lack of interest can do great harm to research, and to the company, in the long run. We may note here that cooperation with the sales department, which was discussed earlier in connection with intracompany relationships, is an important part of the relationship pattern with consumers. By the same token, the research department should take an interest in the trade associations and professional organizations of its industrial consumers. It should publish and furnish them any interesting information resulting from the work of the laboratories. Other means can be found in particular industries which will enable a constructively co-

² *Ibid.*, pp. 37-38.

operative pattern to be built up for both industrial consumers and suppliers.

Those industrial firms which are neither competitors, consumers, nor suppliers are still a part of the total environment and must be considered in establishing the broad aspects of good public relations. Groups, such as the Industrial Research Institute or the National Association of Manufacturers, are composed of representatives of all types of industries. Where possible, research personnel should be encouraged to engage in and assist in this type of activity. The explanation of the technical aspects of the particular business, as well as the offer of assistance in solving the problems of others, is an excellent approach in building sound relations. The more broadly the acquaintanceships and the wider the general esteem for a given research group, the better will it be able to attract and hold capable personnel, and the sounder will be the basis on which the sales department makes sales. For this reason, many companies have found it expedient to furnish speakers, exhibits, etc., from their research departments to trade associations and other companies, even where they have no direct interest in their activities.

Relationships with Professional Societies and Academic Institutions

The closest professional ties of the researcher are those he has with the academic institutions from which he obtained his education, and the professional societies of which he is a member. For this reason, if for no other, the industrial research organization must maintain close contact with academic institutions and professional societies. There are numerous additional reasons for doing so. Universities and technical institutes are sources of capable technical personnel, as well as of ideas and research work of much value to industry. Professional societies provide a medium whereby the research worker can meet with others having similar interests and have a sounding board or listening post for exchange of information. In addition, professional societies usually take the responsibility of assuring that journals exist for the publication of original papers. It is further possible that the universities will undertake direct research on some of industry's problems. This has in general been decried by respon-

sible educators, who point out that the basic contributions of a university should be trained personnel and fundamental knowledge.³ On the other hand, the use of those teaching in the universities as consultants to industrial research organizations may offer advantages to both parties to such arrangements. The industrial group gains knowledge and attitudes which may be extremely valuable, and the university teacher gains experience and contacts which will better enable him to instruct and carry out his more fundamental researches.

In maintaining the best possible relations with professional societies, industry can do no better than to make it possible for the largest practical number of its employees to attend the various national and regional meetings of the societies to which they belong. They should also urge and abet their workers in preparing papers for the groups, and in taking part in the administrative and committee activities so essential for vigorous professional growth. We have already seen that such action on the part of the industrial administrators is useful in increasing good morale, and thus we again find that useful objectives in different areas may be served by the same means. A gesture of good will which is becoming more usual is to grant the local branches of the societies the use of laboratory facilities for meetings. Tours of laboratories, demonstrations of research apparatus, etc., are also useful. Where budgets and policy permit, contributions to joint research efforts of a professional society are also worth while. As in the other areas, the objective is to prove by action that the industrial research department and, by implication, an entire company are working parts of the total environment.

The ways in which industrial research groups can maintain good relations with academic institutions, both on the university and secondary school level, are numerous. In the first place, it must be remembered that almost all professional researchers are graduates of our institutions of higher learning. Second, it needs to be pointed out that the majority of technical assistants in any given laboratory are graduates of local secondary schools. These people will themselves be anxious to maintain contacts with these groups, and any assistance given them will work to

³ F. L. Hovde, "The Universities' Role in the Nation's Research," *Chemical Engineering*, Vol. 56 (January, 1949), p. 105.

the benefit of the organization. Awards, talks, demonstrations, scholarships, fellowships, and gifts are all available to the industrial organization which wishes to obtain a high degree of cooperation, and at the same time serve the general good, of these universities and schools.

Some of these methods may represent considerable expense and may be within the reach of only the larger companies. The measure of the results is not necessarily the money spent in obtaining them. The small company can cooperate just as effectively without having to expend sums beyond its means. By opening its doors to the graduates of the secondary and higher institutions, it will already have taken one step. It is much to its advantage to improve the caliber of the graduates and to assure that its relationships are such that students obtain the feeling that this would be a good company to be associated with upon graduation. Such feelings must be engendered while they are still in school—by the professors, teachers, and administrators with whom they come in contact. This can be done only by a thorough effort to recognize and assist with the problems faced by these individuals.

An excellent means of inculcating good will is to hire, for summer work, students of both secondary schools and universities. This is a common practice in a great many laboratories and is worthy of intensive encouragement. In many cases, the industrial organization can obtain excellent research personnel and technical assistants in this manner, people who by virtue of their summer indoctrination can begin actual work after graduation in a much more mature and useful manner. Even if this is not the end result, the good will of the schools can hardly be obtained in a better manner. Nothing pleases a teacher or academic administrator more than to find jobs for his students, either in summer or after graduation. And the industrial organizations which have cooperated in placing students in summer jobs are apt to find that they have the pick of the graduates.

Where it appears warranted, much is to be gained by the support of fundamental research at the universities, through the offering of either research grants or fellowships. While the work to be done should be in the general field of the particular industry, it should not be so specific as to detract from the general purpose of academic institutions to teach and study funda-

mentals. Nor should the industrial organization attempt to administer the work but leave this matter in the hands of the institution, or an impartial board. Any attempt by industry to direct research in the universities will draw more criticism—and rightly so—than could possibly be overcome by the payment for such work.

Whatever the methods used to encourage good will for industry on the part of academic institutions (and much will depend upon the particular industry and locale of the laboratories and plants), it should be remembered that these are important factors in molding general public opinion. If a particular company is held in high repute by the secondary and higher institutions of its locality, it has a great opportunity to win the good will of the public. This is, of course, quite understandable since the general public has a great affection for learning, and the value of rendering assistance to academic institutions and teachers cannot be overestimated.

Relationships with the General Public

Public relations is a highly specialized art in industry today, and one with which the industrial research department is not ordinarily charged. We have already noted the great number of direct contacts which the research organizations must have and which affect the general regard in which a given company is held. It is not out of place for the research administrator to consider whether or not he is assisting properly in maintaining such relations for his firm. The public with which any company is concerned may be said to consist of two parts, each of which may be consumers of the company's products, which is another story: those who are part of the community or communities in which the company's offices, plants, or laboratories are located, and those who are more remotely situated from actual contact. The former would be, except in most unusual circumstances, a very small part of the total population. It can be considered the most influential, since the company must "live" in direct contact with it. The latter can be influenced only by the more indirect means of advertising, publicity, etc. From the standpoint of the research administrator, this is of less importance. If his relations with other companies, professional

societies, trade associations, academic institutions, and the like are good, he can leave to the professional public relations people the task of indoctrinating the general public with his good works.

The home communities of his company are something else. Here he has the task of providing firsthand information of benefit to his company's relations to the community's residents. His first and possibly most important objective in this regard should be to have a happy and interested working force. Of course, this is essential for other reasons but will as well have an invaluable effect upon the feelings of the community. Every worker serves as a source of information from which the remainder of the community forms an attitude toward the company, and in the research laboratory particularly the feeling of the workers that they are doing important and worth-while jobs can spread very rapidly through a particular population. The professional research worker is usually a respected member of the area in which he lives, and any ideas which he holds concerning his company are apt to be influential. Where research personnel are used to create good will among workers in plants where there are no research facilities, the same condition is apt to prevail for a similar reason. In other words, the use of scientific personnel to "teach" the general public what the activities of a given company are, and what they are contributing to the general standard of living, carries much weight. The scale on which such activities are carried out will of course depend upon the company, ranging from traveling demonstrations covering the entire country to small exhibits and tours of laboratories and plants in single cities.

The direct contact of research workers with the public is not the only means whereby the public is influenced. Just as explanations of what the research department comprises and what its objectives are should be included in material distributed to employees, so they should enter into descriptions, articles, etc., which are available to the general public. Stockholders' reports are an excellent medium for bringing research to the forefront. Although the administrator may not be responsible for writing such publications, he can recommend the inclusion of research material and see that it is properly written.

While the writing of press releases will be left in the hands of those responsible for such activities, the research department

will be slighted, or improperly written about, if those in charge do not provide the public relations experts with suitable material. Much can be accomplished by maintaining close liaison with the publicity department. The same holds for the use of research material in advertising copy. Special pamphlets and house organs issued by the research department may also be distributed to the general public if they contain material of wide interest. Occasionally a technical paper written by a member of the research staff may be redone for public consumption. An important, and not too expensive, method of disseminating information is the provision of motion-picture films of research methods. They find a ready audience among schools, civic clubs, and similar institutions.

If the laboratories are large enough to warrant it, a public relations member may be included on the staff of the research director. In such a case, he would handle all the above activities directly. Otherwise any releases, talks, demonstrations, etc., should be carefully scrutinized by someone experienced in such matters. A condescending or patronizing attitude on the part of research personnel toward the public can do a great deal of harm and certainly will not aid in increasing good will. It has been amply demonstrated in the past that whatever effort is expended in creating good will for industrial enterprises among the general public has been well repaid in increased sales, better labor relations, better applicants for jobs, and moral support in times of crisis.

Relationships with the Government

In general, the researcher's dealings with the patent system and patent legislation are handled through attorneys. Although it is well for him to understand the law of patents and to use this knowledge in improving his company's patent situation, he will not ordinarily have to deal directly with agents of the government in this field. In other cases, too, he will be dealing through intermediaries. Thus, a great deal of research expense should properly be classified as operating expense, and this is an advantage for tax purposes. In some instances there may be a question as to whether a given research expenditure should be considered an asset item and, therefore, capitalizable rather than

deductible as an expense. The research administrator may be called upon to clarify the purposes for which the money was expended and the results obtained, and perhaps even to testify along these lines. The negotiations with government agents will proceed through tax attorneys.

More direct relationships with the Federal government arise if the particular company is involved in governmental contract work. For example, a great many companies are operating under contracts with the Atomic Energy Commission today, and many others are engaged in research for the Department of Defense. Here the research administrator and many research personnel will work directly with employees of the government. While the individuals dealt with are not the government, it is well to remember that the operating departments are composed of individuals. The company usually wishes to maintain a good reputation for conscientious, high-quality research in connection with this type of public work, and to do so, the individuals representing the various agencies must be dealt with in the same manner as the private customers of the firm. To act in any other way is bad practice from the standpoint of public welfare, and bad business as well.

The company should encourage its research personnel to assist in the scientific problems of Federal, state, and local governments. Where possible they should be given the time and opportunity to become members of planning commissions, committees, and other agencies composed of private citizens. This provides good general publicity, as well as specific good will in the governmental agencies concerned. Private industry has a responsibility to make certain that its resources of scientific man power, as well as its productive facilities, are available when needed for the public good. One of the best ways to assure that this responsibility is discharged is by thus encouraging its technical personnel to participate in assisting governmental agencies to solve their problems.

Conclusion

A good research department in an industrial enterprise becomes a productive part of the whole national environment. By influencing, it in turn is influenced, and the product of its

labors is ever more useful in strengthening its company and the national economy. Where it is inefficient or unresponsive to its responsibilities or the regard in which it is held by the entire community, it does great disservice to itself, the enterprise, and the nation.

Broadly, we may conclude that industrial research is susceptible to rational study and understanding. The process represents an amalgamation of scientific creative techniques with economic motivations. The research administrator and the industrial manager have mutual responsibilities in assuring the profitability of the procedures used to solve problems. Management must arrive at a sound policy with regard to project selection, and research administration must objectively scrutinize proposals and the progress of its work. The entire organizational pattern should reinforce the creative abilities of the personnel. The main contribution that a book such as this can make lies in the direction of education toward more objective ways of thinking on the part of the industrial technologist as well as the managers of business. Research in industry cannot be considered apart from its total environment if it is to operate efficiently, and consideration of all the available data and potential methods is as essential here as it is in other research problems. If this study has outlined the need for and some methods of accomplishing this mutual understanding, it will have served its purpose.

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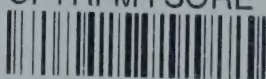
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